A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XX.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR

W. NICHOLSON,

CHARLOTTE STREET, BLOOMSBURY,

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN HILL.

1808.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, R. B.; Joseph Reade, M. D.; Dytiscus; Hemerobius; Mr. Robert Banks; John Gough, Esq.; P.; A. B. C. D.; Lieutenant Henry Kater; Mr. J. Aston; Professor Vince; Mr. Daniel Dering Mathew; Dr. Clerke; Rev. J. Blanchard; Mr. T. Clifton; Mr. J. Wright; Mr. G. H. Willers; John Dickenson, Esq.; Mr. John Martin; Mr. Richard Drew; Mr. John Tatum; a Correspondent.

Of Foreign Works, M. L. F. Lemaitre; M. Tonnelier; M. Theodore de Saussure; Christian Frederic Bucholz; Mr. le Lievre; M. Margueron; Professor Lampadius; M. Vauquelin; M. G. A. Deluc; M. Perperes; M. Planche; M. Depuytren; M. Thenard; Mr. Roloff; Mr. Giulio; Mr. Bergman; Professor Wurzer; M. Collet-Descotils; Mr. Klaproth.

And of British Memoirs abridged or extracted, Sir Joseph Banks, Bart. K. B. P. R. S.; Everard Home, Esq. F. R. S.; Mr. John Middleton; Mr. Joseph Carne; Dr. Alexander Marcet; Major William Lambton; Arthur Biggs, F. H. S.; Richard Antony Salisbury, Esq. F. R. S.; C. H. Parry, M. D.; Mr. G. Irwin; Rev. Thomas Falconer; Mr. William Hardy; Mr. James Dickson, F. L. S.; V. P. H. S.; H. I. Colebrook, Esq.; Mr. F. C. Daniel; Lieutenant John Bell; Humphry Davy, Esq. Sec. R. S. M. R. I. A.; James Smithson, Esq. F. R. S.; Mr. William Garrard; Nevil Maskelyne, D. D. F. R. S.; Mrs. Hannah D'Oyley; Right Hon. Earl of Fife; David Day, Esq.

The Engravings consist of 1. Mr. Middleton's Mode of Printing; 2. Relistian Tin Mine; 3. Singular Strata in a calcareous Mountain near Cressy; 4, 5. Ditto in the Department of Doubs; 6. Draining of the Pond of Citis; 7. Yenite, a new Mineral Substance; 8. Mr. Hardy's Compensation Balance; 9. Compensation Pendulum, by Mr. Henry Kater; 10. An irregular Production of the Cucumber; 11. Mr. Mathew's Scapement; 12. Planche's Apparatus for Succinic Acid; 13. Mr. Wright's artificial Horizon; 14. Mr. Daniel's Life Preserver from Shipwreck; 15. Lieutenant Bell's Method of saving Persons from stranded Ships; 16. Compound Sulphuret; 17. New Properties of Tangents; 18. Radiation and Reflection of Cold; 19. Mr. Richard Drew's Balance Level; 20. Mrs. D'Oyley's Method of rearing Poultry.

TABLE

TABLE OF CONTENTS

TO THIS TWENTIETH VOLUME.

MAY, 1808.

JUNE,

JUNE, 1808.

JULY,

JULY, 1808.

Engravings of the following Objects: 1. Compensation Pendulum; by Mr. Henry Kater: 2. Representation of an Irregular Production of the Cucumber: 3. Mr. Mathew's Scapement: 4. Planche's Apparatus for Succinic Acid.

AUGUST,

AUGUST, 1808.

SUPPLEMENT,

SUPPLEMENT TO VOL XX.

Engravings of the following Objects: 1. Compound Sulphuret: 2. New Properties of Tangents: 3. Radiation and Reflection of Cold: 4. Mr. Richard Drew's Balance Level: 5. Mrs. D'Oyley's Method of rearing Poultry.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1808.

ARTICLE I.

An Attempt to ascertain the Time when the Potato (Solanum tuberosum) *was first introduced into the United Kingdom; with some Account of the Hill Wheat of India. By the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c.* *

THE notes on the introduction of the potato, which it is hoped will not be found uninteresting, were chiefly collected by my worthy and learned friend Mr. Dryander, some of them from authorities not easily accessible. Those on the wheat, though not within the immediate object of this Society, will, I hope, be considered as sufficiently interesting to be laid before them: could we trace the origin of any one of our cultivated plants, it may, and probably will, lead to the discovery of others.

Notes collected chiefly by Mr. Dryander.

The potato now in use (solanum tuberosum) was brought to England by the colonists sent out by Sir Walter Raleigh, under the authority of his patent, granted by Queen Elizabeth, "for discovering and planting new countries, not possessed by christians," which passed the great seal in 1584.

Potato introduced into England in 1586, by Sir W. Raleigh's colonists.

* From the Trans. of the Horticultural Society, Vol. I, Part I, p. 8.

Some of Sir Walter's ships sailed in the same year; others, on board one of which was Thomas Herriot, afterward known as a mathematician, in 1585; the whole however returned, and probably brought with them the potato, on the 27th July, 1586.

First account of them by Herriot.

This Mr. Thomas Herriot, who was probably sent out to examine the country, and report to his employers the nature and produce of its soil, wrote an account of it, which is printed in De Bry's collection of Voyages, Vol. I. In this account, under the article of roots, p. 17, he describes a plant called openawk: "These roots," says he, "are round, some as large as a walnut, others much larger: they grow in damp soil, many hanging together, as if fixed on ropes; they are good food, either boiled or roasted."

Gerard received the roots from Virginia.

Gerard, in his Herbal, published 1597, gives a figure of the potato, under the name of potato of Virginia; and tells us that he received the roots from Virginia, otherwise called Norembega.

First introduction into Ireland.

The manuscript minutes of the Royal Society, December 13, 1693, tell us, that Sir Robert Southwell, then president, informed the fellows, at a meeting, that his grandfather brought potatoes into Ireland, who first had them from Sir Walter Raleigh.

Considered as a delicacy in England in 1597.

This evidence proves, not unsatisfactorily, that the potato was first brought into England, either in the year 1586, or very soon after, and sent thence to Ireland, without delay, by Sir Robert Southwell's ancestor, where it was cherished and cultivated for food before the good people of England knew its value; for Gerard, who had this plant in his garden in 1597, recommends the roots to be eaten as a delicate dish, not as common food.

Conveyed earlier from America to Spain, and thence to Italy.

It appears, however, that it first came into Europe, at an earlier period, and by a different channel; for Clusius, who at that time resided at Vienna, first received the potato in 1598, from the governor of Mons, in Hainault, who had procured it the year before from one of the attendants of the pope's legate, under the name of taratoufli; and learned from him, that in Italy, where it was then in use, no one certainly knew whether it originally came from Spain, or from America.

Peter

Peter Cieca, in his Chronicle, printed in 1553, tells us, chap. xl, p. 49, that the inhabitants of Quito, and its vicinity, have, beside mays, a tuberous root, which they eat, and call papas. This Clusius guesses to be the plant he received from Flanders; and this conjecture has been confirmed by the accounts of travellers, who have since that period visited the country. Mentioned in 1553.

From these details we may fairly infer, that potatoes were first brought into Europe from the mountainous parts of South America, in the neighbourhood of Quito; and, as the Spaniards were the sole possessors of that country, there is little doubt of their having been first carried into Spain, but as it would take some time to introduce them into use in that country, and afterward to make the Italians so well acquainted with them as to give them a name*, there is every reason to believe they had been several years in Europe, before they were sent to Clusius. General inference.

The name of the root, in South America, is papas, and in Virginia, it was called openawk; the name of potato was therefore evidently applied to it on account of its similarity in appearance to the battata, or sweet potato; and our potato appears to have been distinguished from that root, by the appellative of potato of Virginia, till the year 1640, if not longer †. Etymology of the name.

Some authors have asserted, that potatoes were first discovered by Sir Francis Drake, in the South Seas; and others, that they were introduced into England, by Sir John Hawkins; but in both instances the plant alluded to is clearly the sweet potato, which was used in England as a delicacy, long before the introduction of our potatoes; it was imported in considerable quantities from Spain, and the Canaries, and was supposed to possess the power of restoring decayed vigour. The kissing comfits of Falstaff ‡, and other confections of similar imaginary qualities, with which our an- The sweet potato introduced into England earlier. Its reported properties.

* Taratoufli signifies also truffles.

† Gerard's Herbal, by Johnson, p. 729.

‡ "Let it rain potatoes, and hail kissing comfits." Merry Wives of Windsor, Act v, Scene 5.

Parkinson's Paradisus Terrestris, p. 518. Gerard's Herbal, 1697, p. 780.

cestors were duped, were principally made of these, and of eringo roots.

The potatoes themselves were sold by itinerant dealers, chiefly in the neighbourhood of the Royal Exchange, and purchased when scarce at no inconsiderable cost, by those who had faith in their alleged properties. The allusions to this opinion are very frequent in the plays of that age.

Every anecdote that tends to throw light on the introduction, or on the probable origin, of plants now cultivated for use, is certainly interesting, even though it is not quite perfect; I venture, therefore, to add the following.

Small seeds called hill wheat.

Seven or eight years ago, Mr. Lambert brought to me a small paper of seeds, on which was written, "*Hill Wheat;*" I opened it, and found the seeds contained to be scarce larger than those of our wild grasses; but when viewed through a lens, they perfectly resembled grains of wheat.

Produced spring wheat of the ordinary size.

Of these seeds, he was so good as to spare me a few, which I sowed in a garden, the remainder he sowed; our crops very unexpectedly proved to be wheat of the spring kind, and the usual size, the grains of which were nearly, if not quite, as large as those of the ordinary spring wheat.

Came from some part of India.

On this, Mr. Lambert applied to Mrs. Barrington, from whom he had received the seeds, for information of the country from which they came; but she had, among the multiplicity of seeds received by her about the same time, forgot the exact history of them; all she knew was, that they came from India, but from what part of India, she did not recollect.

From the writing on the paper, "*Hill Wheat,*" it is probable they came either from the Peninsula, or from the hilly country, far within land from Bengal, as the province of Bengal itself is a flat alluvial soil, entirely level.

Highly desirable to learn its origin, and whether wild.

The hill wheat, however, is no doubt known to some persons, who either are now in India, or have returned from it into this country; and it is certainly a matter of some importance to know, what they can inform us on the subject of it; especially whether this wheat is a cultivated, or a wild plant; as we shall, if the latter is the case, ascertain two of the greatest desiderata of cultivators; the country where wheat

wheat grows spontaneously; and the nature of the grain in its original state, when unassisted by the fostering hand of man.

II.

Observations on the Structure of the Stomachs of different Animals, with a View to elucidate the Process of converting animal and vegetable Substances into Chyle. By Everard Home, *Esq. F. R. S**.

THE observations on the stomachs of the porpoise†, and of ruminating animals, contained in two former communications, led me to believe, that the fourth cavity of the ruminant's stomach, while the animal is alive, is always divided, in a greater or less degree, into two portions, in one of which is included the plicated structure, in the other, the villous. In some genera, this division is permanent, as in the camel and that tribe; in others only occasional, as in the bullock, deer, sheep, &c. Fourth cavity of the ruminant's stomach divided into two portions.

If this opinion should be found to be true with respect to animals in general, it will throw considerable light on the processes carried on in the stomach, and lead us to conclude, that the food undergoes two changes in it, the one preparatory to the other, and that it is the last of these, which forms the chyle. The food therefore undergoes two changes in it.

With a view to investigate still farther this very interesting subject, I have been led to examine the internal structure of the stomachs of different animals.

In this inquiry it will be found, that the same substances are digested by stomachs varying considerably from each other, and many of these varieties can at present in no other way be accounted for, than by referring them to the general principle, which pervades the structure of animals, making them run into one another by a regular series of minute A regular gradation of form in animals.

* Abridged from the Philos. Trans. for 1807, Part II, p. 139.

† See our last vol.

changes

changes of form, so as to compose one connected chain, from which we derive the fullest evidence of the power and wisdom of their Creator.

Three different structures in the stomach of ruminants,

The stomachs of all ruminating animals have three different structures; the first of these is cuticular; the second has a secreting surface, thrown into folds, on which are seen the orifices of glands; and the third is smooth and more delicate in its texture.

and also of non-ruminants.

In the following account, it will be found that three similar structures are met with in the stomachs of quadrupeds which do not ruminate, and that the gradation between the most complex and most simple stomachs forms a uniformly connected series, of greater extent than has been hitherto supposed.

To complete the view of this subject is too extensive a pursuit for an individual, whose professional duties occupy so large a portion of his time as mine necessarily do. All that can be expected from one so circumstanced is to give a general outline, leaving the minuter parts to be filled up by those who have more leisure, but by no means more zeal, for studies of this kind.

Best mode of examining the stomach, to ascertain its shape and structure.

As the object of the present inquiry is to determine with as much accuracy as possible the shape the stomach puts on, while performing its functions in the living body, and the structure, which belongs to the different parts of its internal membrane, it became necessary to consider what would be the best mode of making such examinations. It was found, that the stomach ought not to be in a distended state at the time of the animal's death, for when this is the case, the air which is let loose, or even the shaking of the contents, elongates or stretches the muscular fibres, so as to enlarge the cavity, and give it a form, by no means natural to it. This partly arises from the weakness of the muscular fibres themselves; but principally from the effect of death upon this organ, which destroys the rigidity of its muscular fibres, so that they become easily elongated, even when much shortened at the time death takes place. It is necessary to mention this circumstance, as it is the reverse of what happens in the voluntary muscles, which are generally known to become rigid at that time, and it accounts for the real form of the

Death destroys the rigidity of its fibres, the reverse of which takes place in voluntary muscles.

the stomach having been much less frequently noticed than was naturally to be expected.

To come at the real form of the stomach, it must be seen recently after death, before its muscles have been disturbed; in this state a gentle and gradual distension with air shows both the permanent divisions of its cavity, if there be any, in the best possible manner, and also any occasional muscular contractions, that are employed during life.

Should be examined soon after death.

The internal membrane is only to be met with in a natural state recently after death, since the secretion from the solvent glands frequently acts upon it, and destroys the surface, and the slightest degree of putrefaction, which comes on very quickly in this cavity, prevents the nicer distinctions of structure from being detected.

Its internal surface soon acted upon after death.

To make an accurate examination of the different parts of this membrane, it is necessary, that its folds should be extended, and the mucus commonly found adhering to it removed; which is most readily effected, and with the least disturbance, by inverting the stomach and gradually distending it; and in this state only can the relative situation of the different structures be ascertained with exactness.

Best method of examining the internal membrane.

In examining stomachs, with the attention directed to all the circumstances above mentioned, it is found, that, in a recent state, the internal membrane is often completely obscured by mucus, which in many instances is inspissated, and puts on the appearance of a cuticular covering, from which it is with difficulty distinguished; in others it resembles a fine villous surface, so very tenacious is its nature; and where the membrane is irregular it adheres with unusual firmness.

Often obscured by mucus.

The internal membrane of most stomachs is found to be considerably more extensive than any of the other coats, and much more so than it appears to be on a superficial examination; for it is not only thrown into longitudinal and transverse folds, but is subdivided by slight fissures into a number of small portions varying in shape and size in different parts of the same stomach, but generally smallest near the pylorus. This appearance was at first mistaken for the real internal structure of the membrane; but when inverted and distended, so as to be put upon the stretch, all these

Much more extensive in general than the other coats.

these disappeared, and it became very thin and smooth. This is seen most readily in the human stomach, and in those of carnivorous animals.

Cannot perform its functions when overdistended.

Such distention enables us to examine the internal structure of parts, but this is not to lead us away from their more natural appearance; since the functions of this membrane could no more go on were it unfolded to a great extent, than the muscular actions of the outer coat, in an overstretched state of its fibres.

Hence a child killed by eating too much.

In proof of this observation, I have known an instance of a child three years old, who, being left alone at dinner, ate so large a quantity of apple-pudding, that it died, which raised suspicion of its having been poisoned. On examination after death, the whole stomach was distended to its utmost extent, and rendered quite tense, which was the only apparent cause of the child's death.

Mr. Home next proceeds to describe the stomachs of a considerable number of animals, his able and minute examination of which is illustrated by several excellently engraved plates; after which he gives the following general observations.

Process of digestion most complex in ruminating animals.

In the stomachs of ruminating animals, the processes the food undergoes before it is converted into chyle are more complex than in any others. It is cropped from the ground by the fore teeth, then passes into the paunch, where it is mixed with the food in that cavity; and it is deserving of remark, that a certain portion is always retained there; for although a bullock is frequently kept without food seven days before it is killed, the paunch is always found more than half full; and as the motion in that cavity is known to be rotatory by the air balls found there being all spherical or oval with the hairs laid in the same direction, the contents must be intimately mixed together; the food is also acted on by the secretions belonging to the first and second cavities; for although they are lined with a cuticle, they have secretions peculiar to them. In the second cavity these appear to be conveyed through the papillæ, which in the deer are conical; and when examined by a lens the focus of which is ½ an inch, they are found to have three distinct orifices, and that part of each papilla next the point is semitransparent.

First stage of this process.

These

These secretions are ascertained by Dr. Stevens's experiments to have a solvent power in a slight degree, since vegetable substances contained in tubes were dissolved in the paunch of a sheep*.

The food thus mixed is returned into the mouth, where it is masticated by the grinding teeth; it is then conveyed into the third cavity, in which it would appear from the gas† let loose, that a decomposition takes place, and thence it is received into the upper portion of the fourth cavity. Second stage.

The changes which are produced on the food in the first three cavities are only such as are preparatory to digestion, and it is in the fourth alone this process is carried on. In the plicated portion the food is acted on by the secretion of the solvent glands; and in this portion of the cavity of the deer's stomach small orifices are seen in the internal membrane leading to cavities, the size of a pin's head, which I consider to be the openings of these glands, since they bear some resemblance to those of other stomachs. In the lower portion the formation of chyle is completed. The 4th stomach the true seat of digestion. Formation of the chyle completed in in its lower portion.

In birds with gizzards the food goes through very similar changes; it is picked up by the bill, which in smaller birds separates the husk from the seed, it then passes into the crop, where it is acted on by the secretions of that cavity, after which it is received into the gizzard, to undergo the same change produced by the grinding teeth of the ruminants; the secretion of the solvent glands is then poured upon it, acting upon the nutritious part before it is spread upon the glandular structure at the orifice of the gizzard, in which last situation it is formed into chyle. Birds with gizzards.

In the whale tribe, the first cavity, although lined with a cuticle, has secretions peculiar to it, and therefore corresponds with the first and second of the ruminants, and with the crops of birds with gizzards: it answers however a Whale tribe.

* *Dissertatio Physiologica inauguralis de Alimentorum concoctione, Autore* Edwardo Stevens, Edinb. 1777.

† Mr Davy and Mr. W. Brande examined this gas, and found it to be inflammable, and not to contain carbonic acid; which establishes a difference between this process and fermentation. Not the fermentative process.

farther

farther purpose, by dissolving its contents sufficiently to prevent the necessity of rumination, or the use of a gizzard. The second cavity performs the same office as the plicated portion of the fourth cavity of the ruminant, and the fourth is that in which the chyle is formed. This complex structure of the stomach in the whale tribe, although it gives it an appearance of great similarity to that of the ruminant, is not at all formed on the same principle, since the additional cavities in the ruminant are to prepare the food for the process of digestion; while in the whale tribe no such preparation is required; but as the fishes they feed upon are swallowed whole, and have large sharp bones which would injure any surface not defended by cuticle, a reservoir became necessary, in which they may be dissolved and converted into nourishment, without retarding the digestion of the soft parts. The very narrow communication between the second, third, and fourth cavities, resembles the opening between the cardiac and pyloric portion in fishes.

Different from the ruminating tribe, though a similarity in structure.

The stomachs of this tribe of animals are therefore introduced here, as being next in order with respect to the complexity of parts, and having by the division of them led me to the present investigation, although it is by no means their proper place, with respect to their mode of digestion.

Animals nearest to the ruminants.

The animals, nearest allied to the ruminants in their mode of digestion, are those which, like them, retain a portion of food in the cardiac extremity of the stomach, that it may undergo a change, before it is submitted to the action of the solvent liquor; and when so hard as to render it necessary, return it again into the mouth, to be masticated a second time.

Hare and rabbit.

Ruminate occasionally.

The hare and rabbit are of this kind; the cardiac portion of the stomach is never completely emptied, and they occasionally ruminate. In proof of both these facts, a rabbit, which had been seven days without food, died, and the cardiac portion of the stomach was found to contain more than half of its usual quantity of contents: they were rather softer than common, and a number, amounting to 50 or 60 of distinctly formed pellets, the size of shot, were collected together in the cardiac extremity, immediately below

below the œsophagus. These could not have been formed at the time of eating, since in seven days the action of the stomach would have destroyed their shape. They must therefore have acquired it by the animal chewing the cud.

This second class of ruminants have no cuticular lining to their stomachs, which may arise from their being more cautious feeders than the others, so that they are not liable to receive into the stomach any thing which can injure its internal membrane. All that portion of the stomach, which corresponds with the first cavity in the true ruminant, has one uniform structure, and is covered with a viscid mucus, but beyond this there are orifices, which I believe belong to solvent glands of a very small size; and toward the pylorus, the glandular appearance is of a different kind; so that in these stomachs the changes the food goes through correspond very closely with those it undergoes in ruminants.

Their difference from the true ruminant.

The next order of animals with respect to digestion consists of the beaver and dormouse. These, both in the shape and general appearance of the stomach, as well as of the teeth, bear a close affinity to the hare; but they have a glandular structure peculiar to them, which seems to correspond with the solvent glands of other animals; and as the dormouse empties its stomach completely, there is reason to believe, that the beaver does so likewise, and that neither of them ruminates, since the regurgitation of the food would be attended with difficulty from the situation of these glandular structures; and it is probable, as they do not ruminate, the increased secretion of a solvent liquor renders it unnecessary.

Beaver and dormouse.

Probably do not ruminate.

The changes the food undergoes in these stomachs are only two; it is acted upon by the secretion from the solvent glands, and afterward converted into chyle by the secretion of those near the pylorus. This is a less complex process than in many of the stomachs not yet taken notice of, and is exactly similar to what takes place in carnivorous animals; it may therefore be considered as a connecting link between the ruminating and carnivorous stomachs.

Link between the ruminating and carnivorous.

After these, which form a regular series from the ruminants, are the stomachs with cuticular reservoirs, in which the

Water rat.

the food is macerated, before it is submitted to the process of digestion. Animals of this kind are the water rat, in which there is a permanent division between the cuticular cavity and the digestive part of the stomach; the common rat and the mouse, in which there is only a muscular one. The cuticular lining is thick and impervious; beyond it is a glandular part, that secretes a mucus found adhering to its surface; and farther on are orifices, which appear to belong to the solvent glands. These animals do not ruminate, and there is a kind of provision in nature to prevent regurgitation of the food. When kept without food for several days they completely empty their stomachs,

Common rat and mouse.

Horse and ass.

The horse and the ass, although animals in all other respects different, correspond so very closely in the structure of their stomachs with the rat and mouse, that their stomachs must be considered of the same kind.

In these the food is rendered easy of solution by remaining in the cuticular reservoirs; it is then acted on by the solvent liquor, and in the pyloric portion converted into chyle.

Kanguroo.

The stomach of the kanguroo, from the peculiarities of its structure, forms an intermediate link between the stomachs of animals which occasionally ruminate, those which have a cuticular reservoir, and a third kind not yet noticed, with processes or pouches at their cardiac extremity, the internal membrane of which is more or less glandular. The kanguroo is found to ruminate, when fed on hard food. This was observed by Sir Joseph Banks, who had several of these animals in his possession, and frequently amused himself in observing their habits. It is not however their constant practice, since those kept in Exeter Change have not been detected in that act. This occasional rumination connects the kanguroo with the ruminant. The stomach having a portion of its surface covered by cuticle, renders it similar to those with cuticular reservoirs; and the small process from the cardia gives it the third distinctive character; indeed it is so small, that it would appear placed there for no other purpose.

Occasionally ruminates.

Stomach occasionally divided into a

The kanguroo's stomach is occasionally divided into a greater number of portions than any other, since every part of

of it, like a portion of intestine, can be contracted separately; and when its length, and the thinness of its coats are considered, this action becomes necessary to propel the food from one extremity to the other. Such a structure of stomach makes regurgitation of its contents into the mouth very easily performed. The food in this stomach goes through several preparatory processes; it is macerated in the cuticular portion; it has the secretion from the pouch at the cardia mixed with it; and is occasionally ruminated. Thus prepared, it is acted on by the secretion of the solvent glands, which probably are those met with in clusters in the course of the longitudinal bands, and afterward converted by the secretions near the pylorus into chyle.

greater number of portions.

Animals with processes at the cardia.

The animals, whose stomachs have processes or pouches at their cardiac extremity, are the kanguroo, hog, pecari, hippopotamus, and elephant.

Pecari.

The pecari's stomach bears the nearest resemblance to those with cuticular reservoirs, having a portion of its surface lined with cuticle; but it only extends to a small distance from the termination of the œsophagus, and is not continued over any part of the great curvature.

Hippopotamus

The hippopotamus's stomach I have never seen, and Daubenton's description and engravings are taken from that of a fœtus; so that the structure of its minute parts is imperfectly known; but there is no doubt of there being a large pouch on each side of the cardiac portion, and there is reason to believe, that no part of the cavity of the stomach is lined with cuticle.

Elephant.

The elephant's stomach is the most simple of this kind. It has no cuticular lining; the elongation at the cardia is only a continuation of the general cavity, distinguished from it by the membranous septa; and the broad one may act as a valve, and occasionally preclude the food from passing.

In these stomachs the pouches at the cardia can only be connected with the preparation of the food, softening it by means of their secretions, or retaining it within their cavities; the other glandular structures are similar to those in the ass and rat, only more conspicuous.

In phytivorous animals the structure of

It is deserving of remark, that the internal structure of the stomachs fitted for digesting vegetable substances, corresponds

the stomach less analogous to that of the teeth than commonly supposed.

responds much less with the kind of teeth, than it has been generally supposed to do. The animals with chissel teeth have no uniformity in the structure of their stomachs; those of the beaver and dormouse being of one kind; the hare's and rabbit's of another; the squirrel's of a third, resembling that of the monkey; the guinea pig's of a fourth, differing from that of the squirrel, in there being a greater disproportion between the thickness of the coats of the cardiac and pyloric portions; the rat tribe of a fifth, which resembles the stomach of the horse and ass, animals whose teeth have a very different form.

Greater analogy between the stomach and weapons of defence.

On the other hand, all the ruminants with horns have one structure of stomach; all those with fighting teeth another, as has been observed in a former paper; also all the animals with projecting tusks have the pouches at the cardia, which appear to be peculiar to them, although there is no connection we yet know of between these weapons of defence and the stomach.

Elephant.

As the elephant's grinding teeth are the best fitted for preparing vegetable food for digestion, so the stomach in its structure approaches nearer to those of carnivorous animals.

The stomachs of which the structure has been hitherto considered belong to animals that feed on vegetables, and chiefly on the leaves, roots, and branches of plants. In the gradation towards carnivorous stomachs, we are next to take notice of those that belong to animals whose principal food is the fruits of trees, which appear to require less preparation for the process of digestion; of this kind are the stomachs of the squirrel and monkey. These in their general appearance resemble very closely the human stomach; at least the few opportunities, which have occurred to me of examining them, have not enabled me to detect any circumstances in which they differ.

Animals that feed on fruits.

Human stomach.

The human stomach appears to be the uniting link between those that are fitted only to divest vegetable substances, and those that are entirely carnivorous; and yet we find in its internal structure it is in every material respect similar to those of the monkey and squirrel, which only digest vegetable productions, and also equally similar to those of

of carnivorous animals. From this it would appear, that many parts of vegetables are as easily digested as animal substances, and require the same organs for that purpose; but others again require a particular preparation, without which they cannot be converted into chyle; of these last the principal are the grasses, which the human stomach is unable to digest. Grasses not digestible by it.

The human stomach is divided into a cardiac and pyloric portion, by a muscular contraction similar to those of other animals; and as this circumstance has not before been taken notice of, it may be necessary to be more particular in describing it. Divided into two portions.

The first instance, in which this muscular contraction was observed in the human stomach, was in a woman, who died in consequence of being burnt. She had been unable to take much nourishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air. The appearance it put on has been already described. The contraction was so permanent, that after the stomach had been kept in water for several days in an inverted state, and at different times distended with air, the appearance was not altogether destroyed. First instance of it observed.

Since that time I have taken every opportunity of examining the human stomach recently after death, and find that this contraction in a greater or less degree is very generally met with. The appearance which it puts on varies: sometimes it resembles that of the ass, so that this effect is not produced by a particular band of muscular fibres, but arises from the muscular coat in the middle portion of the stomach being thrown into action: and this for a greater or less extent, according to circumstances. When this part of the stomach is examined by dissection, the muscular fibres are not to be distinguished from the rest. General: but varies in appearance.

If the body be examined so late as 24 hours after death, this appearance is rarely met with, which accounts for its not having before been particularly noticed. Seldom observable 24 hours after death.

Perrault found a contraction somewhat similar in a lion's stomach, which appeared to him extraordinary, as it was only Lion's stomach similar.

only met with in one instance out of four, that were examined. He gives a drawing of the appearance, but makes no comments on the cause of the contraction*.

Attempt to produce it in a cat.

Finding this contraction was met with, when the human stomach was nearly empty, I endeavoured to produce it in the cat, by having the stomach emptied by means of an emetic a short time before the animal's death. This did not however succeed; for although in the contracted state the line between the cardiac and pyloric portions was very distinct, and the last more contracted than the former, yet upon distending the stomach with air, the middle portion relaxed equally with the rest. The contraction at this part is therefore only to be seen, when these fibres have acted independently of the others; which takes place while the functions of the stomach are going on, but cannot be artificially produced.

Cannot be produced artificially.

Dog.

In examining the stomach of a dog in a contracted state, and afterward when it was distended, the line between the two portions could be distinctly perceived, even after the contraction was destroyed, by the longitudinal folds of the internal membrane of the pyloric portion all terminating there.

Food dissolved in the cardiac portion;

That the food is dissolved in the cardiac portion of the human stomach, is proved by this part only being found digested after death; the instances of which are sufficiently numerous, to require no addition being made to them. This could not take place unless the solvent liquor was deposited there. Mr. Hunter goes so far as to say, in his paper on this subject, "there are few dead bodies in which the stomach at its great end is not in some degree digested."

* La conformation du ventricule étoit particulière, et bien différent en ce sujet de celle, que nous avons trouvés aux autres lions, que nous avons dissequés, où le ventricule étoit semblable à celui des chiens et des chats; ayant un fond ample et large vers l'orifice supérieur qui alloit toujours en s'étrecissant vers le pylore; mais celui ci avoit le fond separé en deux, en quelque façon comme les animaux qui ruminent. Ce forme particulière du ventricule n'étoit qu'en un seul des quatre animaux de cette espèce que nous avons dissequés, sçavoir deux lions et deux lionnes

Mémoires pour servir à l'Histoire Naturelle des Animaux, dressêz par M. Perrault, Fol. Ed. 1676.

That

That the chyle is not formed there, and also that it is completely formed before the food passes through the pylorus, is proved by the result of some experiments of Mr. Hunter's, made upon dogs in the year 1760; and as they were instituted for a very different purpose,—that of determining whether the gastric juice is acid or alkaline,—the results were detailed without any possible bias.

but the chyle formed in the pyloric.

The stomach of seven dogs were examined immediately after death, which took place while digestion was going on; and among other observations the following appear among Mr. Hunter's notes made at the time:

Dogs examined by Mr. Hunter.

"In all the dogs the food was least dissolved, or even "mixed, towards the great end of the stomach, but became "more and more so towards the pylorus; and just within the "pylorus it was mixed with a whitish fluid like cream, which "was also found in the duodenum."

He afterward adds; "It is plain, that digestion is com-"pleted in the stomach, as none of the crude food is found "beyond that cavity; and even within the pylorus there is the "same white fluid, that is met with in the duodenum."

From the result of these experiments, as well as from the analogy of other animals, it is reasonable to believe, that the glands situate at the termination of the cuticular lining of the œsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion: and the muscular contraction retains it there, till this takes place.

Glands that secrete the solvent liquor.

Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions by the action of the muscular fibres at that part where the angle is formed.

Curvature of the stomach accounted for.

It accounts for men occasionally ruminating, a process, which, without such a contraction, could hardly take place. That some men ruminate, the accounts of authors are sufficiently explicit to put beyond all doubt; particularly the instances collected by Peyer from Fabricius *ab Aquapendente* and others, as well as from his contemporaries, in all six or seven instances.

Men occasionally ruminate.

instances. Of these, two were examined after death. In one of them the œsophagus was unusually muscular, but nothing particular was met with in the stomach: in the other, nothing is said of the œsophagus, but the internal surface of the stomach was very rough.

The fact, however, does not rest on these authorities, since a case of this kind has come within my own observation.

An instance observed by the author.

The instance to which I allude, is a man 19 years of age, blind, and an ideot from his birth, who is now alive. He is very ravenous, and they are obliged to restrict him in the quantity of his food, since, if he eats too much, it disorders his bowels. Fluid food does not remain on his stomach, but comes up again. He swallows his dinner, which consists of a pound and a half of meat and vegetables, in two minutes, and in about a quarter of an hour he begins to chew the cud. I was once present on this occasion. The morsel is brought up from the stomach with apparently a very slight effort, and the muscles of the throat are seen in action when it comes into the mouth; he chews it three or four times, and swallows it; there is then a pause, and another morsel is brought up. This process is continued for half an hour, and he appears to be more quiet at that time than at any other. Whether the regurgitation of the food is voluntary or involuntary cannot be ascertained, the man being too deficient in understanding, to give any information on the subject.

The contents not discharged by the first effect of an emetic.

This contraction of the stomach also explains the circumstance of its contents not being completely discharged, by the first effect of an emetic, which only empties the cardiac portion: the contraction preventing the pyloric portion from being emptied till the violence of the straining ceases, at which time relaxation takes place.

Cramp of the stomach.

It may also enable us to account for many symptoms that occur in the diseases of this organ, particularly the violent cramps, to which it is liable: as from the situation of the pain they probably arise from preternatural contractions of these muscular fibres. On the other hand, the indigestion met with in debilitated stomachs may proceed from this part having lost its proper degree of action, and therefore the food is

Indigestion.

not

not retained in it so as to be acted on by the different secretions.

This however is not the place to enter into these subjects; the object of the present investigation has been to collect facts in comparative anatomy, that may throw light upon the conversion of the food into chyle, and to abstain as much as possible from all matters of opinion;—no easy forbearance in going over ground, that has given rise to so many theories, and which the mind cannot contemplate, without forming a variety of conjectures.

Truly carnivorous stomachs resemble the human.

The stomach of the truly carnivorous quadruped appears to be made up of the same parts as the human. In the lynx, the different structures are more strongly marked, the solvent glands are more conspicuous, the pyloric portion is more bent, which renders the division between it and the cardiac more distinct, the muscular coats of the pyloric portion are much stronger, and on its internal surface glands are very obvious, which are not to be observed in the human.

The lynx.

Peculiarities in some.

The stomachs of some carnivorous animals have glandular structures peculiar to them; these are in the pyloric portion; there are also similar glands in the stomachs of some graminivorous animals, as has been already explained. The following may be mentioned as instances of this kind.

Lynx.

In the lynx, a glandular zone surrounds the orifice of the pylorus.

Mole.

In the mole, there is a similar zone.

Stoat and armadillo.

In the stoat, and armadillo, there is a glandular structure near the pylorus.

Sea otter.

In the sea otter, there is a glandular structure extending from the pyloric portion into the duodenum, described in a former paper.

Gradation from carnivorous beasts to birds of prey.

In tracing the gradation from carnivorous quadrupeds to birds of prey, it would have been natural to expect, that the bat, which has wings, and lives on animal food, should form an intermediate link: this, however, is not the case; the stomach of the long-eared bat resembles those of small carnivorous quadrupeds; that of the vampyre bat, which will be found to live on vegetables, has more the appearance of an intestine, and may, from its form, be mistaken for the

Long-eared bat.

Vampyre.

cæcum and colon; in this respect it approaches the kanguroo, and still more closely, the kanguroo rat; its cardiac portion is shorter, and its pyloric longer, than in the stomach of that animal, and there is no valvular structure at the orifice of the cardia.

Ornithorinchus, the only real link between beasts and birds.

The only real link between the stomachs of quadrupeds and birds is that of the ornithorinchus, which, however, is more an approach to the gizzard, being lined with a cuticle, containing sand, and having the same relative situation to the œsophagus and duodenum. The food of this animal is not known; it is probably of both kinds; the papillæ at the pylorus, which appear to be the excretory ducts of glands, are peculiar to it.

Birds of prey.

The stomachs of birds of prey are formed upon the same principle as those of carnivorous quadrupeds, but their cavity is more a continuation of the œsophagus, and the solvent glands are more conspicuous and numerous. Both these differences may be accounted for from their swallowing their prey whole, or nearly so; which requires a more direct passage into the stomach, and a greater quantity of secretion from the solvent glands, than when the food has undergone mastication. The cardiac portion of these stomachs is very distinct from the pyloric.

Snakes, turtles, and fishes.

In snakes, turtles, and fishes, the stomachs have the same characters as in birds of prey, but the cardiac and pyloric portions are still more distinct from each other, and the solvent glands are in general distributed over a larger surface of the cardiac portion.

General conclusions.

From the series of facts and observations which have been adduced, the following conclusions may be drawn.

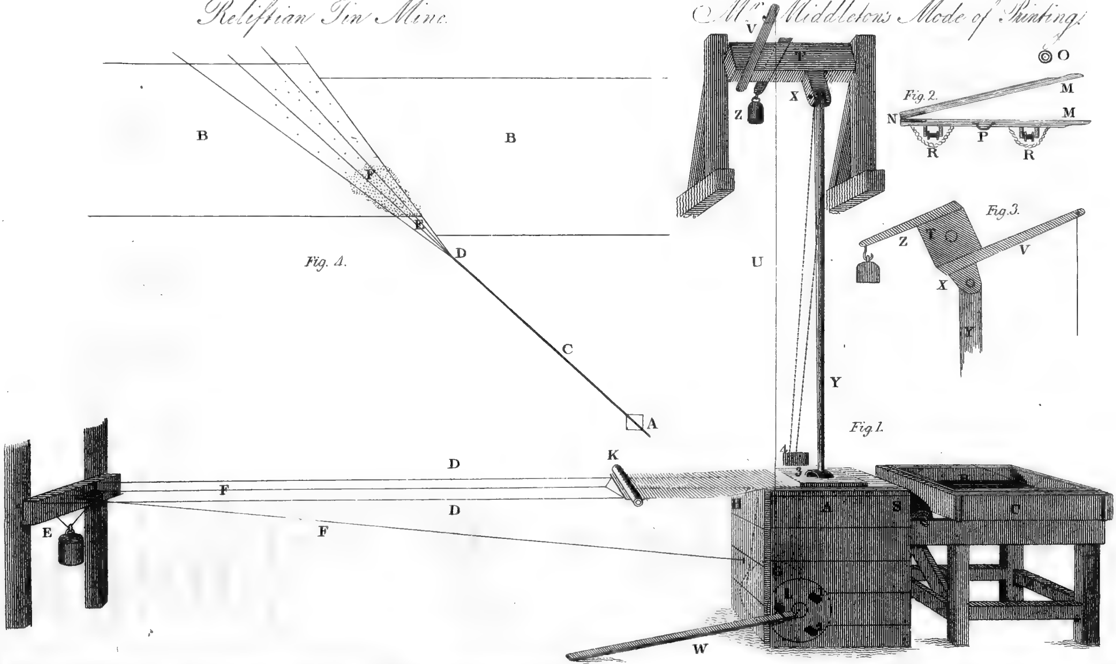
That the solvent liquor is secreted from glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than others.

That these glands are always situate near the orifice of the cavity, the contents of which are exposed to their secretion.

That the viscid substance, found on the internal membrane of all the stomachs that were examined recently after death, is reduced to this state by a secretion from the whole surface of the stomach, which coagulates albumen. This appears to be

Relistian Tin Mine.

Mr. Middleton's Mode of Printing.



be proved, by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

This property in the general secretion of the stomach leads to an opinion, that the coagulation of fluid substances is necessary for their being acted on by the solvent liquor; and a practical observation of the late Mr. Hunter, that weak stomachs can digest only solid food, is in confirmation of it.

Weak stomachs can digest only solid food.

That in converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretion of the stomach, and after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterward conveyed into the pyloric portion, where it is mixed with the secretions peculiar to that cavity, and converted into chyle.

The great strength of the muscles of the pyloric portion of some stomachs will, by their action, compress the contents, and separate the chyle from the indigestible part of the food.

In animals whose food is easy of digestion, the stomach consists of a cardiac and pyloric portion only; but in those whose food is difficult of digestion, other parts are superadded, in which it undergoes a preparation before it is submitted to that process.

III.

Description of a Machine for Printing Paper Hangings. By Mr. John Middleton, *of St. Martin's-Lane.**

BY this machine the printer works with greater facility and dispatch than in the usual way; and the tereboy, who could with great difficulty serve one sieve, can by its means serve two with ease to himself. For this improvement the honorary silver medal was voted to Mr. Middleton by the Society of Arts. The following description shows the nature of this apparatus for facilitating the operations in paper-staining, and the mode of using it both for light and dark grounds.

Advantages of this machine.

* From the Transactions of the Society of Arts, for 1807, p. 135.

Method

Method of printing Light Grounds.

Description of the apparatus.

Pl. I, fig. 1. A, the printer's table covered with a soft blanket. B, the woollen cloth sieve on which the colour is laid and spread by a boy (called the tere-boy) with a hair brush. This cloth sieve is laid upon a leather sieve impervious to wet, and it floats upon some gum liquor, in a wooden vessel C.

D, D, two cords of 36 feet long, stretched from the table A to the other end of the room, and kept tight by a weight at B.

F, F, an endless cord, passing round a grooved wheel G under the table, over a pulley H, in the side of the table, and and over another I, at the other end of the room. Its use is to carry the cross-piece K, called the traverse, which is fastened to it.

L, is a wheel fixed on the same axis as the wheel G, but on the outside of the boarding of the table; it has three pegs projecting about four inches from its face. This wheel is moved by the printer setting his foot on one of the pegs.

Fig. 2, is the traverse on a larger scale. M, M, are two pieces of wood connected by a hinge at N, and when closed are retained in that position by a ring O, put over the ends of them: it is connected with the endless cord, by a staple P on one side, and another staple on the other side, and slides along the cords D, D, by means of two pullies R, R.

Method of printing light grounds.

The operation of printing commences by putting one end of the paper to be printed (which is 12 yards long and 23 inches wide) between the divisions of the traverse (fig. 2), and fastening it there by the ring O. The other part of the paper, except what lies on the printing table, is wound round the roller S. The workman takes up the printing block with his right hand, dips the face of it on the woollen cloth in the sieve, which the tere boy had previously spread with colour, and then places the block upon the paper to be printed, giving it two or three smart strokes with a leaden mall held in his left hand; he then removes the block to supply it with more colour from the sieve; and during this operation sets his foot upon the peg in the wheel; and as he recovers his upright position to bring the block over the table, his foot presses the peg

peg down into the position 2, which, by means of the wheel G, endless cord F, and traverse K, draws the paper forward on the table just the proper distance to print again. When the whole piece is printed, the tere-boy goes to the end of the room, loosens the paper from the traverse, and hangs it up to dry in folds, on loose sticks placed across racks attached to the ceiling.

Method of printing Dark Grounds.

Method of printing dark grounds.

The table and sieve for the colour are the same as in printing light grounds. The difference of printing consists in applying the colour from the block upon the table, by means of a lever, instead of striking the block with a mall; the pressure of the lever forcing a greater quantity of colour upon the paper and in a more even manner,

T, the axle of the lever. Y, the arm (15 inches long) to which the power is applied by means of a rope U, fastened to it, which has a treadle W at its end, for the workman to place his foot upon. X, another arm (6 inches long) to which is jointed Y, a long pole, the end of which is applied to the back of the block 3, when the pressure is given.

Z, an arm on the other side of the axle T, to which a weight is hung to balance the pole Y.

Fig. 3, shows a section of the axle T with the arms V and Z projecting from it, and the manner in which the arm X is connected by a joint with the pole Y; the excellence of this principle depends upon the very great increase of power, which is given by bringing the pole near the centre of the joint or axis.

The paper being placed upon the table as in printing light grounds, and the workman having placed his block, furnished with colour, upon the paper to be printed, he puts his foot on the treadle W, attached to the cord U, takes the pole from behind the piece of wood 4, and applies its end upon the block U, and pressing down his foot makes the impression from the block upon the paper. He then lodges the pole behind the piece of wood 4, to be out of the way; he next removes the block to furnish it again with colour, and draws the paper forward for another impression, by the foot-wheel L, as described in the former mode.

IV.

IV.

An Account of the Relistian Tin Mine. By Mr. JOSEPH CARNE, *in a Letter to* DAVIES GIDDY, *Esq. M.P. F.R.S**.

DEAR SIR, *Penzance, April* 22, 1807.

Chlorite shist cemented by crystallized tin.

WHEN I mentioned the occurrence of pebbles of chlorite shist, cemented by crystallized tin, in the Relistian mine, you expressed a wish to receive a particular account of this novel circumstance.

The mine described.

The Relistian mine is nearly on a level with the surrounding country. The lode has been seen at the depth of 12, 25, 50, 65, 75, 81, and 90 fathoms from the surface. It is of different width in different parts; the extreme width is 36 feet, and in this part it is principally worked. As it extends east and west (which is its due course), its width gradually diminishes, till at the distance of 100 fathoms east it is but 5 feet wide. It is composed (excepting the metallic substances) of shist, chlorite, and quartz. In some parts the shist predominates, and in others the chlorite; the quartz is throughout the smallest component part. The engine shaft (see plan A, Pl. I, fig. 4) is situate 8 fathoms north of the widest part of the lode (B). In sinking the shaft a flookan (C), about 2 inches wide, was discovered, bearing a south-east course, which cut the lode at an angle of 45°; and heaved and disordered it.

At the depth of 12, 25, and 50 fathoms, nothing was discovered in the lode but the cavities from which the ore had been taken away during the former period of working the mine.

At 65 fathoms in depth were found, close to the flookan, a great number of angular fragments of shist, cemented by the same substance.

Flookan divided into four different branches, with a body of pebbles between them,

At the depth of 75 fathoms the flookan (C) became 4 inches wide in the shaft (A), and continued of that size for 10 fathoms; it then became divided into 4 parts or branches (D), each diverging from its former course, and in this state it continued through the lode (B), of which the first 3 feet were

* Philos. Trans. for 1807, Part II, p. 298.

composed

composed of copper pyrites (E), and then was discovered a body of pebbles (F), nearly 12 feet square, extending in width to the extreme branches of the flookan. In this part of the lode the shist greatly predominates; of course the pebbles are generally composed of shist, cemented in some parts by the same substance or chlorite, in others by oxide of tin, which is generally crystallized, and in some of the crevices there is a little copper pyrites. It is singular, that a few pebbles (perhaps not more than half a score) were found of quite a different nature from the others; they were composed of tin in quartz coated with chlorite.

chiefly shist, cemented by shist, chlorite, or oxide of tin.

The pebbles did not continue in a body to the height of more than 2 fathoms; but scattered bunches, and single pebbles, were found 4 fathoms above and 6 fathoms below the place in which they were at first discovered. It is only necessary to add, that the lode has since been worked 15 fathoms deeper than where the pebbles occurred; it there consists for the most part of chlorite formed in a regular manner; not the least trace of pebbles is to be seen, nor indeed of any disturbance in the strata.

I am, dear Sir,

very sincerely yours,

Penzance, Cornwall. JOSEPH CARNE.

V.

An Analysis of the Waters of the Dead Sea and the River Jordan. By ALEXANDER MARCET, *M. D. one of the Physicians to Guy's Hospital. Communicated by* SMITHSON TENNANT, *Esq. F. R. S*.*

THE Dead Sea, or Lake Asphaltite, is situate in the southern part of Syria, near Jerusalem, and occupies an extent of about 60 or 70 miles in length, and from 10 to 20 in breadth. This lake has been from time immemorial celebrated on account of the intense saltness of its waters, which

Dead Sea.

* Philos. Trans. for 1807, Part II, p. 296.

is

is such as to prevent either animals or vegetables from living in it, a peculiarity from which it has derived its name. It appears, that this saline quality has existed in the earliest ages; for independently of the frequent allusions made to it in the Scriptures, we find it described by several ancient authors, amongst others by Strabo*, who wrote during the reign of Augustus, by Tacitus†, and by Pliny‡. Amongst modern travellers, Pococke§, Volney||, and others, have noticed and described this singular spot.

Only analysis of it.

But although the most obvious peculiarities have for a long time been in some degree known, the only chemical analysis I have been able to find on record is that which was published in the "*Mémoires de l'Académie des Sciences*" for the year 1778, by Messrs. Macquer, Lavoisier, and Sage. The names of Lavoisier, and of his two distinguished associates, might appear to render any further investigation of the nature of this water superfluous; but whoever has perused the paper in question must be convinced, that these gentlemen, however correct in their general statements, neither attained that degree of accuracy of which modern analysis is susceptible, nor did they bestow on the subject that share of attention, which is indispensable in minute analytical experiments.

Water brought home by Mr. Gordon.

The gentleman to whom I am indebted for the specimen of the water of the Dead Sea, which is the subject of this paper, is Mr. Gordon of Clunie, who recently travelled in that country, and undertook, not without some difficulty and danger, an excursion from Jerusalem to this remarkable lake. There he himself filled and brought to Sir Joseph Banks a phial, containing about one ounce and a half of this water, carefully corked, and in a state of perfect preservation. The same gentleman brought also in another phial, somewhat larger, a specimen of the River Jordan, which runs into the Dead Sea, without having any outlet, so that the river might be expected to hold in solution ingredients analogous to those of the Lake itself. These specimens Sir Joseph put into the hands of Mr. Tennant, for examination. But knowing that I

* Strabonis *Geogr.* vol. ii, p. 1107.
† Tacitus, lib. v, *Hist.* cap. vi.
‡ Plinii lib. v, cap. xv, and xvi.
§ Pococke's *Travels* in 1743, ii, p. 34.
|| Volney, i, 281.

was

was engaged in similar researches, Mr. Tennant was so obliging as to entrust me with this analysis, and to afford me frequent opportunities of availing myself of his assistance in the course of the inquiry.

Being possessed but of a small quantity of this water, a further supply of which could not easily be procured, I was anxious not to waste any considerable portion of the specimen by preliminary trials. With this view, I began by making a variety of comparative experiments on artificial solutions, in order to ascertain the accuracy of different modes of operating; and knowing by Lavoisier's analysis, and also by the general effects of reagents applied to minute quantities of the water, what were the principal ingredients which I might expect to find in it, I made solutions, the contents of which I had previously ascertained with precision, so that by analysing these solutions in different ways, I had an opportunity of judging of the degree of accuracy that could be expected from a variety of methods. Some of these trials I shall briefly relate; for although not strictly belonging to the particular analysis in question, yet I conceive, that they may be of some general use, in pointing out the most eligible method to be pursued in inquiries of this kind. Indeed it must be confessed, that the minute chemical examination of any individual substance requires so much time and patience, that to obtain a knowledge of that substance only would seldom appear a sufficient inducement to such a laborious undertaking, was it not always more or less connected with other useful collateral objects.

Preliminary observations.

Sect. I.

General Properties of the Dead Sea.

1. One of the most obvious peculiarities of the Dead Sea-water, is its specific gravity, which I found to be 1·211, a degree of density scarcely to be met with, I believe, in any other natural water. The circumstance of this lake allowing bodies of considerable weight to float upon its surface was noticed by some of the most ancient writers. Strabo, amongst others, states that men could not dive in this water, and in going into it, would not sink lower than the navel; and Pococke, who bathed

General properties of the water.

bathed in it, relates that he could lie on its surface, motionless, and in any attitude, without danger of sinking. These peculiarities, which I at first suspected of being exaggerated, are fully confirmed by Mr. Gordon, who also bathed in the lake, and experienced all the effects just related.

2. The water of the Dead Sea is perfectly transparent, and does not deposit any crystals on standing in close vessels.

3. Its taste is peculiarly bitter, saline, and pungent.

4. Solutions of silver produce from it a very copious precipitate, showing the presence of marine acid.

5. Oxalic acid instantly discovers lime in the water.

6. The lime being separated, both caustic and carbonated alkalies readily throw down a magnesian precipitate.

7. Solutions of barytes produce a cloud, showing the existence of sulphuric acid.

8. No alumine can be discovered in the water by the delicate test of succinic acid combined with ammonia.

9. A small quantity of pulverized sea salt being added to a few drops of the water, cold and undiluted, the salt was readily dissolved with the assistance of gentle trituration, showing that the Dead Sea is not saturated with common salt.

10. None of the coloured infusions commonly used to ascertain the prevalence of an acid or an alkali, such as litmus, violet, and turmeric, were in the least altered by the water.

SECT. II.

Preliminary Experiments to ascertain the Composition of the Salts concerned in this Analysis.

'omparative
periments.

Having satisfied myself by these preliminary experiments, that the Dead Sea contained muriate of lime, muriate of magnesia, and selenite, and having no doubt both from the taste of the water, and from Lavoisier's statement*, that it contained also common salt, I proceeded to the comparative experiments above mentioned.

The first indispensable step was to ascertain with accuracy the proportions of acid and base in the three muriates just named.

* Macquer, Lavoisier, and Sage, discovered the three muriates, but overlooked the small quantity of selenite.

named. This I had already done in the course of a more general inquiry, which I began some time ago in conjunction with Mr. Tennant, and which has been of great use to me on the present occasion. But as the particulars of that series of experiments may probably be published at some future period, I shall now confine myself to such general statements as immediately belong to my subject.

1. The composition of muriate of lime was ascertained by pouring a known measure of muriatic acid on a piece of pure marble of known weight, and more than sufficient to saturate the acid. The remaining portion of marble being then weighed, and the solution evaporated and heated to redness, the proportions of acid and earth were easily deduced. But in order to draw such an inference, it was necessary to ascertain with precision the quantity of pure lime in a given weight of marble, which, from a number of experiments performed with great care by Mr. Tennant and myself, appeared to be 56·1 parts of lime in 100 of marble. From a great variety of trials, made with considerable attention, and with due allowance for any accidental circumstances, muriate of lime appeared to consist of 50·77 parts of lime, to 49·23 of muriatic acid.

Muriate of lime consists of 50·77 lime, 49·23 acidt

Marble contains 56·1 lime

2. To ascertain the proportions of earth and acid in muriate of magnesia, required a synthetic process somewhat different. To a known weight of pure magnesia perfectly calcined, a known quantity of acid* was added, and after the whole of the magnesia was dissolved, the remaining portion of acid was saturated by marble. From the loss sustained by the marble, and the known proportions of acid and magnesia used, the composition of muriate of magnesia (supposed perfectly free from water) was deduced, and the proportions resulting from several careful trials were 43·99 parts of magnesia, to 56·01 of muriatic acid.

Muriate of magnesia contains of magnesia 43·99 acid 56·01.

3. Muriate of soda was analysed by various methods. But the

Muriate of

* By a *known quantity* of acid is meant as much acid as will dissolve a known weight of marble. In all these experiments the quantities of acid were not weighed, but measured by means of a peculiar apparatus, and the real weights or intrinsic quantities of acid, corresponding to the measures in question, were easily deduced from the results above mentioned.

soda, 54 soda, 46 acid.

the only one which I shall now relate consisted in precipitating the acid by a solution of silver from a known weight of muriate of soda, and inferring the proportion of acid and alkali from the quantity of luna cornea obtained. This however required a previous exact knowledge of the proportions of acid and silver in luna cornea. In order to ascertain this point, a known quantity of acid was precipitated by nitrate of silver, and the weight of the luna cornea, after being melted and heated to redness, indicated 19·05 parts of acid to 80·95 of oxide of silver. The composition of common salt, calculated from these data, proved to be 46 parts of acid to 54 of soda.

Muriate of silver, 80·95 oxide, 19·05 acid.

Sect. III.

Comparative Analysis of artificial Solutions.

Artificial solutions of these salts analysed.

I shall not enter into all the particulars of the various analyses of artificial solutions, resembling the water of the Dead Sea, which directed me in the choice of the method which I ultimately adopted. But it may be proper to state, in a summary manner, the principal means which were tried, and their respective defects and advantages.

These artificial mixtures all contained the three muriates above mentioned, but in each of them the small quantity of selenite was altogether disregarded.

1. The first of these solutions was evaporated to dryness, and the residue exposed for near an hour to a red heat in a platina crucible pretty closely covered. The object of this was to drive off the acid from the magnesia (muriate of magnesia being decomposable by heat), and after separating this earth from the other salts by means of distilled water, to precipitate the lime by carbonate of ammonia, and to obtain the muriate of soda by evaporation to dryness. But I soon found, that the complete decomposition of muriate of magnesia by heat, under these circumstances, was extremely difficult, if not impossible; and accordingly the results obtained from this method indicated considerably less magnesia and proportionally more lime, than the solution really contained. The quantity of common salt was tolerably accurate.

Heat did not completely decompose the muriate of magnesia.

2. From

2. From another similar solution the lime was precipitated by oxalate of ammonia; the magnesia was separated by heat in an *open* crucible, and the common salt was obtained, as before, by evaporation and exposure to a low red heat. The result was satisfactory both as to the lime and magnesia; but as the separation of the latter could only be completed by long continued heat, in an open vessel, I found the muriate of soda materially reduced by sublimation, and was therefore obliged to abandon this mode of proceeding.

The muriate of soda diminished by sublimation.

3. From a third artificial solution, the lime was precipitated by oxalate of ammonia, the magnesia by carbonate of ammonia recently prepared, and the sea salt was obtained as usual by evaporation and desiccation in a low red heat. The object of this mode of operating was to supersede the necessity of applying a red heat in the first instance. But I was again disappointed; for the magnesia was but imperfectly precipitated; and in order to separate the last portions of this earth, it was necessary to calcine the last residue containing the muriate of soda, which gave rise to the same objections as in the former experiments.

A 3d trial defective.

4. The last and most successful method consisted in dividing the artificial solution into two portions. From one of these the muriatic acid was precipitated by nitrate of silver, and its quantity ascertained. From the other the lime was separated by oxalate of ammonia, and the magnesia by caustic potash *; and the respective portions of acids belonging to each of these earths being calculated, the quantity of muriate of soda was inferred from the remaining quantity of acid.

Most successful method.

This method afforded remarkably accurate results. The only objection to it seems to be, that the muriate of soda being only estimated, and not actually obtained, if any errour be made either in the estimation of the acid or in the separation of the lime and magnesia, these errours must also ultimately affect the computation of the muriate of soda, without allowing any immediate means of detecting them.

Only objection to it

* Or, by carbonate of ammonia. In this case the precipitation of magnesia is not so perfect; but the precipitate falls down more quickly, and the separation of any remaining portion of this earth may be ultimately completed by heat.

This

in great measure removed.

This objection, however, is in a great degree removed, by a comparison of the two portions of the solution, from one of which the common salt can be obtained undecomposed; and the present method has this additional advantage, that the quantity of acid is a sort of check, which, when connected with some other point of comparison, prevents any gross errour in the computation of the earths from escaping notice.

This plan being very similar to that which I actually followed in the analysis of the water of the Dead Sea, it may be worth while to mention the summary results of the comparative experiments which decided me in its favour.

The artificial solution contained:

Actual contents of the solution.

	Salts.		Acid.
Muriate of lime······	8·17 grains		4·02 grains.
Muriate of magnesia··	26·10	=	14·62
Muriate of soda······	25·00	=	11·50
	59·27*	=	30·14

And the contents inferred by the foregoing method, were:

Contents given by the analysis.

	Salts.		Acid.
Muriate of lime······	8·14	=	4·01 grains.
Muriate of magnesia··	25·62	=	14·35
Muriate of soda······	25·47	=	11·72
	59·23	=	30·08

Sect. IV.

Analysis of the Dead Sea Water.

Analysis of the Dead Sea water.

I now come to the actual examination of the water of the Dead Sea, the particulars of which will be found much shortened by the preceding observations.

* These happened to be very nearly the real proportions of salts in the Dead Sea; yet this coincidence was a matter of mere accident; for when I mixed up the ingredients, I was led to suppose from Lavoisier's paper, that their proportion in the Dead Sea was very different from that which I afterward ascertained.

1. 20 grains of this water (the whole supply of which amounted only to 540 grains) were put into a glass capsule, and slowly evaporated in a water bath, by means of an appropriate apparatus, the temperature of the capsule being constantly kept within 5 degrees of 180°. The object of this experiment was simply to know the weight of the solid contents of the water, dried under various degrees of heat, and to observe the appearances produced by evaporation. After a few hours, and when the residue had ceased to lose weight, the saline mass, whilst still warm, appeared in the form of a white semitransparent incrustation, which yielded to the touch, being soft, and of a pulpy consistence. In cooling it became hard, and of a much more opaque white colour. When examined with attention, the borders of this mass were found covered with small cubic crystals, and the same appearance was observed, though less conspicuously, in the centre under the saline incrustation, when in the state of semifusion just described. On standing in the air for some time, the white opaque mass gradually absorbed water from the atmosphere, and returned to a liquid state. The 20 grains of the water, thus evaporated and dried at 180°, weighed, whilst still warm, 8·2 grains.

By evaporation.

2. The same saline mass, being afterward exposed in a sand bath to the temperature of 212° Fahrenheit, was reduced to 7·7 grains. Hitherto not the least smell of muriatic acid was perceived, nor did any decomposition appear to take place.

3. But having raised the heat about 15° higher, the residue, after a few minutes, was found reduced to 7·4 grains; and on redissolving it, a few insoluble white particles appeared floating in the solution, showing an incipient decomposition in the muriate of magnesia.

It appears from these experiments, that 100 parts of the Dead Sea water yield 41 of salts dried at 180°, and 38·5 dried at 212°*. What proportion these quantities bear to the

* If the quantity of materials upon which these results are founded should appear too small, I would observe, that, if the bulk of salt be considerable, it is impossible to dry it accurately, owing to the crust which forms on the surface, and prevents the escape of moisture. But at

the same salts, when perfectly deprived of water, will be seen from the subsequent results. I now pass on to the chemical examination of the water.

By nitrate of barytes.

4. To 100 grains of the Dead Sea water a few drops of muriate of barytes being added, a precipitate was obtained, which, after being well washed and exposed to a low red heat on a piece of laminated platina, weighed 0·09 grain, which, allowing for the unavoidable loss attending the manipulation of such very minute quantities, may safely be called 0·1 grain. This residue, on being heated with fluat of lime, instantly ran into a globule, and was evidently sulphate of barytes.

By muriate of silver.

5. To another portion of the Dead Sea water, weighing 250 grains, a solution of nitrate of silver being added till it ceased to produce any precipitate, a quantity of luna cornea was obtained, which after careful edulcoration and exposure to a red heat, weighed 163·2 grains, a quantity equivalent, according to the proportions above stated (sect. II, 3), to 31·09 grains of real acid.

Muriate of ammonia added.

6. To the remaining solution a little muriate of ammonia was added, in order to remove the unavoidable small excess of silver, and this new precipitate was separated and well edulcorated.

Oxalate of ammonia.

7. The clear fluid, which had been much increased in bulk by these edulcorations, being concentrated to about 3 ounces, a strong solution of oxalate of ammonia, warm, but not nearly boiling*, was added to it, by which a precipitate was obtained, which collected and washed with the usual precautions, and after deducting 0·076 grains of lime† for

at any rate no perfect accuracy can be relied on respecting this kind of limited desiccation, as its completion depends in a great degree on the shape of the vessel, the thickness of the stratum of salt, &c.

* The precipitates of lime by oxalate of ammonia subside more readily, if the solutions be used warm; but when concentrated and heated to the boiling point, this test acts also in some degree on magnesia, a circumstance which in the present instance was to be particularly avoided.

† The proportion of lime in selenite, and of acid in sulphate of barytes, are taken from a paper of Mr. Chenevix, in Nicholson's Journal, Vol. II, in which they are stated to be 56·4 of lime in 100 parts of selenite, and 24 parts of acid in 100 parts of sulphate of barytes.

the

the 0·136 grains of selenite belonging to 250 grains of the water, yielded 4·814 grains of pure lime $=$ 4·66 grains acid $=$ 9·48 grains muriate of lime.

I should not omit mentioning, that the method which I used in all my experiments to ascertain the quantity of pure lime in oxalate of lime, consisted in driving off the oxalic acid by a low-red heat, and adding to the calcareous residue, then converted into a subcarbonate, a known quantity of muriatic acid more than sufficient to dissolve the whole lime. A piece of marble of known weight was afterwards added to take up the excess of acid, and from these data the quantity of lime was calculated with great precision.

8. The clear solution containing nitrate of magnesia, nitrate of soda, and a small excess both of oxalate and muriate of ammonia, and amounting in bulk to about 4 ounces, was exposed to the heat of a lamp for concentration; but in a few minutes the mixture became turbid and began to deposit a white powder, which, from former observations, I supposed to be oxalate of magnesia. To this solution concentrated to between 2 and 3 ounces, and still warm, I added carbonate of ammonia with excess of pure ammonia. A considerable precipitation immediately appeared, and the mixture became opaque and milky. The next morning, however, the fluid had become quite transparent, and instead of a white impalpable precipitate, I found clusters of perfectly pellucid crystals spread over the bottom of the vessel, with distinct interstices between them.

Subcarbonate of ammonia,

This salt was no doubt an ammoniaco-magnesian carbonate; and the remaining solution, although still containing, as will presently appear, a vestige of magnesia, was so far free from it, as not to have its transparency disturbed by caustic potash. These crystals, after being well washed in distilled water, were exposed to a gentle heat to drive off the ammonia, in consequence of which they crumbled down into a white impalpable powder, exactly resembling common carbonate of magnesia. This powder being then treated, and its quantity estimated, in a way similar to that which had been employed with the lime; and being increased by the addition of about 0·5 of a grain of a similar precipitate (which had escaped the action of the carbonate of ammonia

 and

and was obtained from the last remaining solution by evaporation and calcination), amounted to 11·10 grains of pure magnesia = 14·15 grains of muriatic acid = 25·25 grains of muriate of magnesia.

9. The muriate of soda was next estimated from the 12·28 grains of muriatic acid found to remain after substracting the sum of the two portions (4·66 grains and 14·15 grains) belonging to the lime and magnesia, from the 31·09 grains, or sum total of acid. These 12·28 grains gave according to the proportions before mentioned (sect. II, 3) 26·69 grains of muriate of soda.

10. From these several results brought into one view, and the salts being supposed heated to redness, 250 grains of the Dead Sea water appear to contain,

Contents of 250 grains of the water,

	Salts.		Acid.
Muriate of lime ······	9·480 grains		4·66 grains
Muriate of magnesia ··	25·25	=	14·15
Muriate of soda ······	26·695	=	12·28
Sulphate of lime ······	0·136		
	61·561		31·09

or of 100.

And therefore 100 grains of the same water would contain,

	Grains.
Muriate of lime ··············	3·792
Muriate of magnesia ···········	10·100
Muriate of soda ··············	10·676
Sulphate of lime ··············	0·054
	24·622

Sect. V.

2d. analysis,

Second Analysis of the Dead Sea Water by a Method somewhat different from the former.

by a somewhat different mode.

In the mode of proceeding just related some small loss in the earths might naturally be suspected to have taken place in consequence of the previous separation of the acid and indispensable edulcorations. Besides, the muriate of soda being necessarily decomposed by the first part of the process, the analysis could not have been considered as quite satisfactory,

satisfactory, had not the common salt been procured unaltered by some other process.

1. In order to obtain these points, 150 grains of the water were treated, with regard to the lime and magnesia, exactly as in the former analysis; but in this case, the acid, instead of being actually separated by silver, was only calculated from the former estimation (sect. IV. 5).

2. The result proved perfectly agreeable to my expectation. It yielded a little more lime and magnesia than the former analysis, but this excess was scarcely perceptible. With regard to the muriate of soda, I was able actually to procure by evaporation as much as 13·1 grains of this salt, the actual quantity of which, inferred as in the preceding analysis, was 15·54 grains, a difference easily accounted for by the necessity of heating the salt to redness for its ultimate separation.

3. On summing up the contents of these 150 grains of the water, they appeared to be as follow:

Contents of 150 grains, by this analysis.

	Salts.	Acid.
Muriate of lime	5·88 grains	2·89 grains.
Muriate of magnesia	15·37 =	8·61
Muriate of soda	15·54 =	7·15
Selenite	0·08	
	36·87	18·65

And consequently the proportions of these salts in 100 grains of the water would be: Proportions in 100.

	Grains.
Muriate of lime	3·920
Muriate of magnesia	10·246
Muriate of soda	10·360
Sulphate of lime	0·054
	24·580

The coincidence of these results with those of the former analysis was such as I could scarcely have expected to increase by further trials. The last statement, however, I consider as the most accurate of the two.

It

General result. It may therefore be stated in general terms, that the Dead Sea water contains about one fourth of its weight of salts supposed in a state of perfect desiccation; while, as I observed before, if these salts be only desiccated at the temperature of 180°, they will amount to 41 per cent of the water. This great difference between the two states of desiccation depends on the great affinity which muriates, particularly that of magnesia, have for water. Muriate of soda is scarcely at all concerned in this difference: for I found, not without surprise, that 100 grains of artificial cubic crystals of muriate of soda, being fused and heated to redness in a platina crucible, lost at most half a grain.

Proportions of Macquer and Lavoisier. In the analysis of Macquer and Lavoisier, the solid contents of the Dead Sea are estimated at about 45 per cent of the water, and in the proportions of nearly 1 part of common salt to 4 of muriate of magnesia, and 3 of muriate of lime; proportions widely different from those which I had obtained. But their mode of operating, which they candidly relate, was so evidently inaccurate with regard to the separation and desiccation of the salts, and in general so deficient in the estimation of quantities and proportions, that these eminent chemists cannot be considered as having aimed, in this instance, at any thing like an exact analysis.

It may be observed also, that these gentlemen found the specific gravity of the water 1·240 instead of 1·211, as I have stated it to be; but it appears, that their specimen had suffered some evaporation previous to their experiments, since they found crystals of common salt in one of their bottles, which could not have happened without evaporation.

The proportions above perhaps rather too small. Besides, the specimen which I examined was, I understand, brought from a part of the lake not more than two miles distant from the mouth of the Jordan, a circumstance which may perhaps account for its being somewhat more diluted, than it might be found in other parts.

Sect. VI.

Analysis of the Water of the River Jordan.

Water of the Jordan. As I had scarcely two ounces of this water, and as it contained but a very small proportion of saline ingredients, it would

would have been in vain to aim at analysing it with strict accuracy. Yet I thought it worth while to endeavour to form as exact an estimation of its contents as I could, on account of its connection with the Dead Sea, into which, as was observed before, it pours its waters, and appears to remain in a stagnating state. This specimen was brought from a spot about three miles distant from that where the river enters the Dead Sea.

Apparently pure,

From the perfect pellucidity of this water, its softness, and the absence of any obvious saline taste, I was led to suppose, that it was uncommonly pure, and could in no degree partake of the peculiar saline qualities of the Dead Sea. But I was soon induced to alter my opinion by the following results.

but analogous to that of the Dead Sea except in strength.

1. The same chemical reagents, as were used to ascertain the general properties of the Dead Sea water, being applied to this, produced analogous effects. The same three muriates and even the vestige of selenite were distinctly discovered; and this resemblance became more striking in proportion as the water was concentrated by evaporation.

Apparently contains $\frac{1}{300}$ only of its solid contents.

2. 500 grains of this water being evaporated at about 200°, the dry residue weighed exactly 0·8 of a grain. This makes the solid ingredients amount only to 1·6 grain in 1000 grains of the water, a singular contrast with the Dead Sea, which contains nearly 300 times that portion of saline matter. As the water was concentrating, a few white particles were perceived on its surface, and a few others gradually subsided. When dried, the residue appeared in the form of a white incrustation, the upper edge of which exhibited great numbers of very minute crystals, which from their saline taste, and their cubic shape, discoverable by the aid of a microscope, were evidently common salt.

3. Distilled water being thrown on this residue, a minute portion of it remained undissolved, and on pouring an acid on this substance, a distinct effervescence was produced, showing the presence of carbonate of lime.

4. From the clear fluid a precipitate was obtained by oxalate of ammonia, which, dried but not calcined, weighed 0·12 of a grain.

5. From

5. From the remaining clear solution a magnesian precipitate was produced by ammonia and phosphoric acid, which, after driving off the ammonia by heat, weighed 0·18 of a grain.

6. The solution had suffered too many alterations to allow me to separate, with any degree of accuracy, the muriate of soda; but from a variety of circumstances, I thought it not unlikely, that it would have been found pretty nearly in the same proportions, with respect to the other salts, as it exists in the Dead Sea.

The Dead Sea perhaps the same water concentrated by evaporation.

The inference I drew from this was, that the River Jordan might possibly be the source of the saline ingredients of the Dead Sea, or at least that the same source of impregnation might be common to both. This inquiry, however, would require a much more correct knowledge both of the proportions of the salts, and of local circumstances, than I have been able to obtain.

VI.

An Account of the Measurement of an Arc on the Meridian of the Coast of Coromandel, and the Length of a Degree deduced therefrom in the Latitude of 12° 32′. *By Brigade Major* William Lambton.

(Concluded from Vol. XIX, p. 317.*)*

Reductions of the hypothenuses.

THE reductions from the hypothenuses to bring them to the horizontal level were made by numbering the feet from the old chain as they were measured, viz. by calling 32 chains 3200 feet, which would be 3200·115 feet by the new chain; but this would produce no sensible errour in the versed sign of a very small angle, and on that account these decimals were not taken into the computation, which was thought less necessary, since the whole deduction did not amount to three inches. Neither was any notice taken of the different heights of the hypothenuses or levels one above another, as that difference was too trifling to affect a length of thirty or forty chains. The base has therefore been considered

sidered at the same distance from the centre of the earth, before it was reduced to the level of the sea, and the perpendicular height of the south extremity, which I have considered as nearly the general height, has been taken for that purpose. That perpendicular height was obtained by comparing the south with the north extremity, and the height of the latter was determined by observations made at the race-stand and on the sea-beach, where allowance has been made for the terrestrial refraction. The following is the manner in which it has been determined:

Determination of the height above the sea.

On the top of the race-stand, the under part of the flag on the beach was observed to be depressed 9′ 30″; and at the beach, the top of the race-stand was elevated 7′ 15″. When the instrument was on the platform of the race-stand, the axis of the telescope was on a level with the top of the railing, which was observed from the beach. But at the beach the axis of the telescope was four feet below the part of the flag which had been observed.

The horizontal distance from the station on the stand to that on the beach is = 19208 feet. Then as 19208 : 4 :: Rad : tan. 43″, which must therefore be added to the observed depression of the flag. Hence 9′ 30″ + 43″ = 10′ 13″ is the depression of the axis of the telescope on the beach, observed from the race-stand.

Now the station on the beach is nearly at right angles to the meridian, therefore, by allowing 60957 fathoms to the degree, 19208 feet will give an arc of 3′ 9″ very nearly, which is the contained arc. And the difference between the depression and elevation being 2′ 58″, we have $\frac{3' 9'' - 2' 58''}{2} = 5''{\cdot}5$ for the terrestrial refraction. Hence, since the observed elevation of the stand, *plus half* the contained arc, would give the angle subtended by the perpendicular height of the stand above the telescope at the beach, were there no refraction, we shall have $7' 15'' + \frac{3' 9''}{2} - 5''{\cdot}5 = 8' 44''$ for the true angle subtended by the perpendicular height, which being taken as tangent to the horizontal distance and radius, we have R : tan. 8′ 44″ :: 19208 : 48·797 feet the height required. But the axis of the telescope on the beach was determined, by levelling down

down to the water, to be 21·166 feet above the sea. Which, added to the above, give 69·963 feet for the perpendicular height of the top of the stand above the level of the sea.

Now the top of the race-stand was determined by levelling to be 31·25 feet above the north extremity of the base; which, taken from the other, leaves 38·713 for the north extremity of the base above the sea, which extremity being, by the table, 22·96 feet above the south extremity, we shall have 15·753 feet from the perpendicular height of the south extremity of the line above the level of the sea; and from this height the length of the base has been reduced.

The angles of elevation and depression were taken by the circular instrument, from a mean of several observations, and the errour of collimation was corrected by turning the transit over, and the horizontal plate half round. But the weather was rather dull during the whole of these operations.

Major Lambton then proceeds to give the particulars of the measurement of his base line, commencing in lat. 13° 00′ 29·59″ N., and extending 40006·4418 feet south-westerly, making an angle of 10′ 36″ with the meridian.

Commencement of the operations from the base. The large theodolite.

Properest stations selected.

After the completion of the base line, there remained nothing of importance to be done until I received the large instrument, which arrived in the beginning of September. I had however made an excursion down the sea coast, as far as Pondicherry, for the purpose of selecting the properest stations for determining the length of a meridional arc. This and the measurement of a degree at right-angles to the meridian I considered as the first object of this work: I accordingly lost no time in proceeding to accomplish these desiderata.

Theodolite.

The instrument above alluded to was made by Mr. Cary, and is in most respects the same as that described by General Roy in the Philosophical Transactions for the year 1790, with the improvements made afterwards in the microscopes,

scopes, and in an adjustment to the vertical axis, by which the circle can be moved up or let down by means of two capstan screws at the top of the axis. These are mentioned in the Philosophical Transactions for 1795, in the account of the trigonometrical survey. By sinking the circle on the axis, it is better adapted for travelling, and when the microscopes are once adjusted to minutes and seconds, on the limb of the instrument, the circle can always be brought back to the proper distance from them. Great attention however is necessary in bringing the axis down, so that the wires in each microscope being fixed at opposite dots on the limb, they may coincide with the same dots when the circle is turned half round, or made to move entirely round, and in a contrary direction to what it had been moved before; which latter method has been recommended by the maker. This circumstance respecting the axis should be most scrupulously attended to before the adjustment of the micrometers begins, so that when by arranging the lenses in such a manner that ten revolutions of the micrometer may answer to ten minutes on the limb, and therefore one division to one second, the circle can always be brought to its proper height, by trying the revolutions of the micrometer.

It has however been found from experience, that unless in cases of very long and troublesome marches, it is not necessary to sink the axis. The carriage being performed altogether by men, there is not that jolting which any other mode of conveyance is subject to, and as I found, that a considerable time was taken up in adjusting the axis before the revolutions of the micrometers could be brought to their intended limits, I therefore laid it aside, unless under the circumstances above mentioned.

Semicircle of the transit telescope.

The semicircle of the transit telescope is graduated to 10′ of a degree in place of 30′, which was the case with the semicircle described by General Roy, and the micrometer to the horizontal microscope applied to this semicircle, making one revolution in two minutes, and five revolutions for ten minutes on the limb; and the scale of the micrometer being divided into sixty parts, each part is therefore two seconds of the circle.

Errour of the semicircle.

A number of experiments have been made for determining

ing the errour of the semicircle, and to ascertain the place of the fixed wire in the horizontal microscope, so as to divide the errour. It has appeared in the event, that the telescope being in its right position, (that is, when the limb and microscope were on the left hand,) and the fixed wire placed at zero on the semicircle, when the circle or limb of the theodolite was turned 180° in azimuth, and the telescope turned over, the fixed wire was then distant from zero on the opposite part of the arc by a mean of a great many observations 2′ 57″, the half of which is therefore the errour. This half was carefully set off from zero by the movable micrometer wire, and the fixed one brought to coincide with it. On the right application of this errour, there will be 1′ 28″·5 to add to the elevations and subtract from the depressions. The observations for determining this quantity were repeated at different times, and under the most favourable circumstances; the adjustments of the whole instrument being frequently examined, and the level applied to the telescope reversed at most of the observations. For the

Line of collimation.

line of collimation, as these corrections depend on having a well defined object, I fixed a bamboo upwards of a mile distant from the observatory tent, and tied round it several narrow stripes of black silk, one of which was near the horizontal wire when the axis of the telescope intersected the staff after being brought to a level by the bubble. Then the instrument being adjusted, and the telescope directed to the bamboo being perfectly level, and the wire of the micrometer in the piece brought to the intersection of the cross wires, the angular distance to the mark on the bamboo was measured by the runs of that micrometer, and the wire brought back to the point of intersection of the other wires. The circle was then turned half round and the telescope reversed or put again into the same Ys. The levelling adjustment was then made, and the angular distance from the intersection of the wires to the black mark again taken, half the difference between which and the former was of course the errour of collimation. This errour was repeatedly reduced till it became very small, half by the finger screw of the clamp to the semicircle, and half by the adjusting screws to the levelling rods. After that, the remaining errour

rour was repeatedly examined and found to be 2″·36 to be substracted from the elevations and added to the depressions when the telescope is in the ordinary position, or when the semicircle and microscope are on the left hand; but *vice versa* when in the contrary position. These errours of the semicircle and line of collimation being opposite, the result from comparison will be, "That when *elevations* or *depressions* are taken with the semicircle, 1′ 26″ must be added to the *former*, and substracted from the *latter*."

And that when the elevations and depressions are taken by the micrometer in the eye piece 2″·36 must be *deducted* from the *elevations* and added to the depressions.

Micrometer. The micrometer in the focus of the eye-glass of the transit telescope is the same in all respects as the one mentioned by General Roy, that is to say, the circle or scale is divided into one hundred divisions, and there is a nonius fixed to the upper part of the telescope, which defines the revolutions of the micrometer as far as ten for the elevations, and ten for the depressions. Several experiments were made with the same marked bamboo, for ascertaining the value of these divisions, and it was found, that seven revolutions and 61·4 divisions were equal to ten minutes on the limb of the semicircle, so that one division was equal to ·788 of a second.

Remarks on the angles taken. Having given tables of all the angles, Major Lambton adds. The angles have been taken with much care, and I believe with as much accuracy as the nature of such a process admits of; difficulty, however, very frequently arose from the haziness of the weather, which rendered the objects at the very distant points extremely dull, and occasioned some irregularity in the angles. Whenever that happened, the observations were often repeated, and in case any one, in particular, was different from the other so much as ten seconds, it was rejected till the three angles of the triangle had been observed. If the sum of these angles was near what it ought to be, no further notice was taken of it; but should the sum of the three angles be nearer the truth by taking it into the account, and that there appeared an irregularity in the other two observed angles, I have made it a rule to take each observed angle as a correct one, and divide

divide the excess or defect between the other two, and then compute from the given side the other two sides; and after doing the same thing with each of the angles successively, a mean of the sides thus brought out was taken, which, to certain limits, will always be near the truth. I then varied the selection of the observed angles, rejecting such as I had reason to doubt; and by correcting them, and computing the two required sides of the triangle, those which gave the sides nearest to what had been brought out by the other method were adopted, let the errour be what it would. This, however, has rarely happened; and when it did, great precaution was used; and no angle was rejected, without some reason appeared to render it doubtful.

In correcting the observed angles to obtain those made by the chords, I have used the formula given by the Astronomer Royal, in his demonstration of M. de Lambre's problem, which appears in the Philosophical Transactions for 1797. The spherical excess is of course had from the well known method of dividing the area of the triangle in square seconds, by the number of seconds in the arc equal to radius, where the number of feet in a second may be had by using the degree as has been commonly applied to the mean sphere, or the mean between the degree on the meridian and its perpendicular. This being of no farther use than to check any errour that might happen in computing the corrections for the angles.

Observations by the Zenith Sector for the latitude of Paudree *station, and the station near* Trivandeporum; *and the length of the celestial arc.*

Zenith sector. The zenith sector, with which these observations have been taken, was made by Mr. Ramsden, and is the one alluded to by General Roy, in the Philosophical Transactions for 1790, being then unfinished. The radius of the arc is five feet, and the arc itself is of that extent to take in nine degrees on each side of the zenith. It is divided into degrees, and smaller divisions of 20′ each, which are numbered. Each of these last is again subdivided into four, of 5′ each. The micrometer, which moves the telescope and arc, is graduated to seconds, and one revolution moves the arc

arc over 1′ 10″ 08‴, but the scale being large, a small fraction of a second can be easily defined. The construction, and improvements to the zenith sector, are so well known, that a minute description of it here would be unnecessary. It will therefore suffice to say, that as far as so delicate an instrument can be managed in a portable observatory, or travelling tent, which never can offer the advantages of a fixed, well contrived building, I have every reason to be satisfied with it.

Observations. The time I commenced observing at Paudree station was during the heavy part of the monsoon, which occasioned frequent interruptions: and although I had intended observing by at least three fixed stars, I only succeeded to my satisfaction in one, which was Aldebaran. With that star I had a fortunate succession for about sixteen nights; some few of these observations, being less favourable than the others, were rejected.

During the time I was at Trivandeporum, near Cuddalore, the weather was settled and serene, and the nights perfectly clear, so that I had an unlimited choice of stars, but having been successful with Aldebaran, I chose that star for determining the length of the arc.

Manner of observing, and adjusting the instrument. As I consider the celestial arc more likely to be erroneous than any terrestrial measurement, I have thought it necessary to give some account of the manner of observing and of adjusting the instrument, for, after two years experience, I have found, that, notwithstanding the great powers of the zenith sector, extreme delicacy and attention are requisite, to render the observations satisfactory. The following method of adjustment I have always practised. After having brought the vertical axis nearly to its true position by the adjusting screw at the bottom, or so that the wire of the plummet would bisect the same dot when the telescope was moved to the opposite side, or half round on the axis, I then examined whether the dot at the centre of the horizontal axis was bisected, and whether the wire moved in the vertical plane clear of the axis; for unless it be perfectly free, all the observations will be false. When I had bisected the dot, I either took out the microscope and looked obliquely, or did the same by a magnifying glass, and by that

that means I could discover the smallest parallax. If it admitted being brought nearer to the axis, it was done; but I found from experience, that it was more eligible to leave the wire at a sensible distance, than to bring it very near. Having satisfied myself in this particular, I examined with the microscope again in front, moved the wire freely in the vertical plane, and then bisected the dot. The telescope was then moved, so that the wire was brought over the dot zero on the arc, and the same precaution used with respect to the wire moving free of the arc; and here, as well as above, I found it best to allow a sensible distance between the wire and the arc.

Adjustment of the microscope.

The microscope by which the upper dot in the horizontal axis is examined being fixed by the maker, the axis of vision is of course at right angles to the vertical plane, and will meet that plane in the centre of the axis; but the lower microscope is movable, and requires care to fix it so as to have the wire in the axis of vision, and be free from the effects of parallax; this I have done by moving it along the brass plate in front of the arc, till the wire appeared free from curvature, and then adjusted the dot. In these late observations, I have generally made the final adjustment by the light of a wax taper, for the wind being sometimes high and troublesome, I found there was much irregularity in the observations, until I adopted that method. I therefore closed the doors and windows of the observatory tent, so as to have a perfect stillness within. The distance of the wire from the axis and the arc is likewise better defined by a taper by noticing the shadow in moving the light to the right and left.

Fixing the instrument.

In fixing the instrument for the star, great care was taken to have it placed in the meridian, which was done by a mark at near the distance of a mile (generally one of my small flags), the polar star having been previously observed by the large theodolite for this purpose. The telescope was then moved in the vertical, till the wire of the plummet was at the nearest division on either limb to the zenith distance of the star, which could always be nearly known. The micrometer, having been put to zero, was firmly screwed, and the dot on the limb carefully bisected, the instrument was turned

turned half round; the adjustment examined and corrected, if necessary. This being done, the degrees and minutes, &c., on the arc were noted down, as was also the particular division on the micrometer scale, at which the index stood, and the fractional part of a division in case there was any. In this state every thing remained to within fifteen or twenty minutes of the time the star was to pass, when I repaired to the tent, and again examined whether the wire bisected the dot; if it did not, the instrument was again adjusted to the same dot, and the horizontal axis also examined by the upper microscope, all this being done, the sector was placed in the meridian.

Observation of the star.

When the star entered the field of view, the micrometer was moved gently till the star was near the horizontal wire, but not bisected till it came near the vertical, that the micrometer might not be turned back, but continue moving in the same direction. This I did to avoid any false motion in the micrometer screw, and I was led to this precaution by the repeated experiments I had made in examining the divisions on the arc, for it sometimes happened after moving the arc over one of the divisions till the wire bisected the next dot; and then turning it back again, that the index of the micrometer was not at the same second, but had passed over it perhaps one, and sometimes two seconds; but by moving over the next five minutes in the same direction, the number of revolutions and seconds were always what they ought to be, to some very small fraction. This anomaly, however, only happened in some situations of the screw, and to avoid any errours arising therefrom, I adopted the above method.

Caution respecting the micrometer.

Zenith distance taken on both parts of the arc.

The zenith distance of the star being now had, on one part of the arc or limb, after the same process had been gone through the next night, with regard to the adjustment, the zenith distance was taken on the other part of the arc, by turning the instrument half round on its vertical axis. The mean of these two was therefore the true observed zenith distance, and half the difference was the errour of collimation. For applying these to the purpose in question, the mean of the zenith distances being corrected for refraction, the declination of the star for each of these nights was corrected for

 nutation,

nutation, aberration, &c., to the time of observation, and the mean of the two taken for determining the latitude.

In this manner has the whole series of observations been continued, by turning the sector half round every night, for the purpose of observing on opposite parts of the arc, and each compared with its preceding and succeeding one. In pursuing this method, it was unnecessary to notice the errour of collimation for any other purpose than as a test to the regularity of the observations; for until they became uniform, no notice was taken of the zenith distances, concluding that there had been some mismanagement, or some defect in the adjustment.

Degree of lat. between 12° and 13° N. 60495 fath. Deg. of long. in 12° 32′ 61061 fath.

After major Lambton had made out his account of the meridional arc, he completed the measurement of a degree perpendicular to the meridian in latitude 12° 32′ nearly, derived from a distance of upward of fifty-five miles, between Carangooly and Curnatighur, two stations nearly east and west from each other. The final results of his computations are, that the degree on the meridian in this latitude is 60495 fathoms nearly, and the degree perpendicular to it 61061 fathoms nearly.

VII.

An Account of some New Apples, which, with many others that have been long cultivated, were exhibited before the Horticultural Society, the 2d of December 1806. *By.* Mr. ARTHUR BIGGS, *F. H. S.*

The apple one of our best fruits.

OF all the different fruits, that our island affords, none can be brought to a higher degree of perfection, with so little care and trouble, especially in its southern counties, than the Apple. For a proof of this, I hope it will not be deemed presumptuous in me to refer to the catalogue below, every variety of which I had the honour of exhibiting to the Horticultual Society, at our meeting in December last. Having been flattered by the wishes of many gentlemen then present,

present, to give some account of such as are new, and by what culture they have been produced in such perfection, I cannot but attempt it, though very inadequate to the task, for almost every hour of my life has been employed in following the instructions of others, and when I have deviated from them, with a view to improvement, I have seldom been able to write down the result of my experiments with any satisfaction to myself.

Apple trees should not stand too close, nor their branches be too thick.

Beside the sorts of apples lately exhibited, the garden of Isaac Swainson, esq., my indulgent master, contains a number of others, which are less valuable. When I mention that I am cutting these away as the better trees advance, and thinning the branches of the latter also as they require it, I perhaps tell all that is to be told upon the subject; for I have found nothing of more consequence to the health of the apple tree than plenty of light and air. The instructions of the late Mr. Philip Miller, on this head, are so pointed, and I see so many apple trees smothered either by their own branches or those of other trees, that I cannot do better than quote his words. After directing the standard trees to be planted at the distance of 40 feet every way, and the dwarfs at that of 20 feet, he says, "I am aware how many enemies I shall raise by retrenching the great demand, which must of necessity be made in the several nurseries of England, if this practice be adopted, but as I deliver my sentiments freely on every article, aiming at nothing more than the information of my readers, so I hope there will be found none of my profession of such mercenary tempers, as to condemn me for telling the truth, though it may not always agree with their interests."

Large ones should be 40 feet, dwarfs 20 feet distant.

May be planted close, to be thinned as they grow.

I feel no fear in referring to this great gardener's work, because all the principal nurserymen, who now supply the public in the vicinity of London, are men of too much liberality to recommend a less distance, than the above; and in the present opulent state of this country, the original price of the trees is comparatively so trifling, that if any one plants double the number which ought to remain, he will be repaid more than a hundred fold in the few years that the alternate trees are suffered to stand. This is a practice, therefore,

 which

which I have not scrupled to recommend: but, after all, whether a gentleman plants many or few trees, his future success and gratification depend principally upon the judgment of his gardener, in choosing such trees in the nursery, as have been grafted from *bearing branches;* and if I thought myself authorized to give any hints to our nursery-men, it would be relative to the selection of their grafts and buds, not only in the apple tree, but every sort of fruit tree, about which they are in general too careless.

Should be grafted from bearing branches.

Grafts to be selected with care.

I must now observe that the apple tree will grow readily by cuttings, and that trees raised in this way, from healthy one year old branches, with blossom buds upon them, will continue to go on bearing the very finest possible fruit, in a small compass, for many years. Such trees are also peculiarly proper for forcing, by way of curiosity or luxury, and I believe that they are less liable to canker than when raised by grafting, though I am unable to assign any reason for it. I have more than once experienced this in the *golden pippin*, cuttings of which have remained seven years in perfect health, when grafts taken not only from the same tree, but from the very branch, part of which was divided into cuttings, cankered in two or three years. Accident, which brings to light so many useful things, first taught me this practice; some cuttings, that I had stuck into the ground for marks of annual flowers, having all made roots. The soil was loamy, and the summer proved so wet and cold, that many bunches of grapes in a large greenhouse, which I could not prevail upon the gentleman I then served to be at the expense of thinning with scissars, rotted when green.

Apple trees raised with advantage from cuttings.

These well adapted to forcing,

and continue in health longer than grafts.

This discovered by accident.

The soil at Twickenham is light, and inclined to sand rather than loam, in which the apple tree will ripen its fruit earlier and more completely than in a stiffer soil, but it will not last so long. Young seedling plants will also produce their blossoms and fruits in a shorter period in such soil. Our trees being originally placed too near each other, I have transplanted several into other quarters with very great success, even after they had attained a considerable magnitude. In doing this, I was careful to preserve every root possible both great and small, to have the ground where they were to be

Effects of soil on the apple tree.

May be transplanted large.

be planted ready open to receive them, so that their roots were only exposed to the air a few minutes, disposing their fibres as horizontally as possible, and not too deep. The months of September and October should be preferred for transplanting any large tree, watering it well if showers do not fall the same day: if the leaves are not pulled off, it will make fresh roots immediately, or at all events be more disposed to push them forth in spring. I constantly tread the ground exceeding firmly with my feet, in separate layers of about an inch, so as to render staking unnecessary, a practice which if performed so as to have any real effect is very expensive, but which too frequently does more mischief than good.

Treading the ground well on the roots preferable to staking.

Of the varieties of the apple cultivated in Mr. Swainson's garden, which ripen early, I can especially recommend, the summer pippin, Devonshire quarrington, summer traveller, bland rose, summer pearmain, red colville, marigold, Kirk's incomparable, Evan's valuable, nonsuch.

Early ripening apples.

Of the autumn and winter varieties, perhaps all those which follow are valuable, especially such as are marked with a star, and those marked with a cross are new.

Autumnal and winter varieties.

*Norfolk storer, *Norfolk beaufin, Norfolk paradise, Holland pippin, embroidered pippin, striped Holland pippin, *lemon pippin: as this variety is beginning to canker in many gardens, there is no doubt that it is old, and has been introduced from the continent, probably Normandy: for a gentleman who was at Rouen, during the last short peace, saw it there in abundance.

*Ribston pippin, New Town pippin, *golden pippin, Marmail pippin, French pippin, Kirton pippin; Wyken pippin, Fern's pippin, London pippin, *Kentish pippin, New Town late pippin, mathematic pippin, †William's pippin, Whitmore's pippin, New York pippin, raspberry pippin, cat's head, *king of pippins, nonpareil codling, Cowring's queening, *flower of Kent, Selleswood's reinette, *Holland berry, golden mundi, margill, nutmeg apple, royal russet, golden russet, Pile's russet, Clifton crab, *Minchin crab, French crab, Herefordshire pearmain, Loan's pearmain, Holt's pearmain, Kentish reinette, lady's thigh, pigeon's egg, Tolworth court, spice apple,

apple, quince apple, hall door, *transparent pippin, *golden reinette, golden royal, †Bigg's nonsuch, †flat green, †false beaufin, summer breeding, cœur pendu, †Minier's dumpling, †Padley's pippin, †oval apple, †green pyramid.

Description of the best.

To give a complete history of each of the new apples above mentioned is out of my power: they have all been raised by other gardeners, from whom we may rather expect it: in the mean while, however, the following descriptions will perhaps suffice to make those which appear to me the best, more known.

William's pippin.

William's pippin. Size, from 2 inches to 2½ inches long. Colour, pale yellow, with a little red on the sunny side, and here and there a spot. Shape, somewhat conical, scarcely longer than broad, deeply umbilicated at the stalk, which is short, hollow at the top; the leaflets of the calyx, though black and dry, still remaining more perfect than in many. Flesh, pale, yellow, soft, excellent to eat ripe from the tree, baking and roasting well, till Christmas.

Padley's pippin.

Padley's pippin. Size, from 2 to 3 inches in length. Colour, rich yellow, generally very finely laced all over with a pale rough starry bark, if I may use the term. Shape, oval, about the stalk flat, or often a little prominent on one side, not much depressed about the calyx, which is more obliterated than in many others, perhaps from this circumstance. Flesh, firm and juicy, of a rich perfumed and poignant flavour, in high perfection all December and January. I am inclined to think this the very best of our new apples.

Bigg's nonsuch.

Bigg's nonsuch. Size, from 2 to 3 inches in length. Colour, deep yellow, striped and variegated with red on the sunny side. Shape, and general appearance, somewhat like the nonsuch, but broader at the base, moderately depressed about the foot-stalk, very hollow at the top, where the leaves of the calyx remain long and rolled back. Flesh, pale, yellow, soft, and excellent to eat ripe from the tree; roasts and bakes well till Christmas.

Minier's dumpling.

Minier's dumpling. Size, from 3 to 3 inches and a half in breadth, but not so long. Colour, deep green, and very dark red next to the sun; which, together with its spherical shape, more contracted at the top, and swelled into a few imperfect angles,

angles, give it some appearance of the Norfolk storer, but there are darker green lines on the north side which distinguish it from all the apples I know. It is depressed about the stalk, which is long and stout enough for so large an apple. The calyx is nearly obliterated by the time the fruit is ripe, which is not till Christmas, or after. It is most valuable for boiling or baking till April, and even to eat at the end of the season; its flesh firm, high flavoured and juicy.

VIII.

On the Cultivation of the Polianthes Tuberosa or Tuberose. By RICHARD ANTHONY SALISBURY, *Esq. F.R.S. &c*.*

THE charms of Horticulture, in every civilized nation, have been acknowledged by men of all ranks, from the highest prince down to the lowest cottager. While the graver duty of the historian has been simply to commemorate the calm and innocent delights which it affords, the holy mythologist has exalted it as the sole employment of our first parents in Paradise; and poets have embellished their most enchanting verses with its productions: so that to offer a long and laboured panegyric upon any single branch of it, to a Society instituted for the express purpose of encouraging them all, would, in the emphatic language of an old writer, be like vainly attempting to paint the lily, add a perfume to the violet, or gild refined gold. The field before us, moreover, is no less extensive than that of the whole globe, which is in fact one immense garden, covered with vegetables common to every animal that exists; but Providence has in infinite wisdom allotted to man the proud preeminence over all; his wants, if he is not indolent, being invariably first supplied. In those earlier stages of society, however, when the ground was first cultivated, it must have

Art of gardening.

* Abridged from the Trans. of the Horticultural Soc. vol. I, p. 41.

been

been inconceivably difficult to exclude various animals, both carnivorous and herbivorous, from the immediate precincts of human habitations; driven as they now are from every populous country, we can form but a very imperfect idea of their tremendous power and strength in warmer climates, while thinly inhabited. Hence the progress, even of Agriculture, was in all probability for a long period slow and interrupted: years and years must have elapsed, before her younger and more delicate sister, Horticulture, ventured to appear; though, to plant a clump of *bananas,* which would give immediate shade, and to perfume the surrounding air with the fragrance of an *orange* grove, independent of the fruit these two vegetables afford, must have been natural, one would think, to many a savage of finer feelings, the moment his residence became fixed.

subsequent to agriculture.

Tuberose recommended as profitable.

To leave the language of fancy for that of fact, I know no ornamental plant, which seems to me more deserving of cultivation in the warmer soils of this kingdom, or that would repay the labour attending it with greater profits, than the *tuberose.*

First account of this flower.

The first account that I find of the *tuberose,* is in l'Ecluse's History of Plants, where it appears that on the 1st of December 1594, he received a specimen of it, in very bad condition, from Bernard Paludanus, a physician at Rome, to whom it was sent by the celebrated Simon de Tovar, of Seville. It certainly had not then been many years in Europe, and Linné, in his Hortus Cliffortianus, on this head refers us to Plumier's Genera Plantarum, p. 35, who says it was first brought by Father Minuti, from the East Indies, into the senator Peiresc's garden at Boisgencier, near Toulon. It is much more probable, however, that it was introduced at an earlier period, and from America, for no author describes it as wild in the East Indies. Loureiro only found it cultivated in the gardens of Cochin China, and Rumph says it was unknown in the Island of Amboyna, till the Dutch carried it there from Batavia, in 1674. On the contrary, Kamel informs us, that it was brought to the Island of Luzone, by the Spaniards, from Mexico; and Parkinson, in 1656, tells us, that the plants, which he describes as two species, "both grow naturally in the West Indies, whence being first

Supposed from the East Indies.

More probably from America.

Natives of the

first brought into Spain, they have from thence been dispersed unto divers lovers of plants." The senator Peiresc, as may be learnt from Gassendi, was only fourteen years old in 1594, when Simon de Tovar had already cultivated it at Seville, and according to Redoutè, it was not planted in his garden at Boisgencier, by Father Minuti, till 1652, whom that author makes to have brought it from Persia. I only infer, however, that he travelled from Hindostan over land. Redoutè moreover asserts, that the authors of the Flora Peruviana found it wild in America, but in the work itself they say, cultivated in gardens. Hernandez' evidence, however, I think, takes away all doubt about the matter: he says, "provenit in frigidis et temperatis regionibus, veteri incognita mundo," and as the *agave*, to which the *tuberose* is more immediately allied, is also a native of Mexico, I am fully of opinion that it is indigenous there. West Indies.

L'Ecluse an accurate observer.

The description given by the venerable l'Ecluse, of his specimen, half dried and battered by the journey, with only the lowest flower of the spike expanded, affords a memorable instance of his accuracy and discernment. The size of the stem, insertion and figure of the leaves, and their hempy texture, are particularly noticed; the shape of the corolla, with its general similarity to that of the *Asiatic hyacinth*, but in consistence rather to that of the *orange*, is next remarked; and having no knowledge of the root to guide his judgment, but what he derived from Simon de Tovar's appellation of *Bulbus Indicus florem album proferens hyacinthi Orientalis æmulum*, he guesses it may possibly belong to the same genus with the *bulbus eriophorus*, or *Peruvian hyacinth*, though not without some doubts raised by its stem being covered with leaves, and its tubular corolla. Two years afterward, these doubts were corroborated by his receiving roots both from Simon de Tovar, and the Comte d'Aremberg, which by August were full of leaves; and I think it worth noticing, that his figure of the plant appears evidently to have been made up from the original specimen sent by Bernard Paludanus, and one of those growing roots, which he expressly mentions did not flower: he concludes with observing,

serving, that if it is still to remain in the genus, it may be called *hyacinthus Indicus tuberosâ radice.*

Origin of the name.

From this latin phrase, no doubt, our silly appellation of *tuberose*, and the more accurate French name, *tubereuse*, originated; but in the East Indies it is distinguished by the poetical title of *sandal malum*, or *intriguer of the night;* in Spain, where at the period of this plant's being discovered it was the fashion to give both places and things religious names, it is called *vara de S. Josef.*

East Indian name.

Figures of it. Vallèt. Swertius.

Soon after l'Ecluse's figure, an excellent one by Vallèt the embroiderer came out at Paris in 1608, and both these were copied and published as different species, by Swertius, in his Florilegium. An original figure, which has great merit for that day, though not equal to Vallèt's, next appeared in the Theatrum Floræ, my edition of which, I believe the earliest, bears the date of 1622; it shows many roots flowering in one pot. From Ferrarius's pompous book on the culture of flowers, we learn it was still regarded as a rarity in the Barberini gardens, at Rome, in 1633, but that it increased abundantly, and was taken out of the ground every year in March, to separate the offsets. Our countryman Parkinson, more than half a century after its being first described by l'Ecluse, is the next author who treats of this plant; but valuable as many of his quaint observations still are to the horticulturist, his account of the *tuberose* does him little credit; he makes two species of it, saying, he thinks l'Ecluse never saw the first, though he owns "some do doubt that they are not two plants several as of greater and lesser, but that the greatness is caused by the fertility of the soil;" his figures are wretchedly copied from Swertius, and by his calling it the *Indian knobbed jacinth*, it appears not to have been known here then by its modern name. Gaspar Bauhin, with his usual carelessness, also takes it up as two species from Swertius, and even the learned Ray seems to have known as little about it in 1693, adding, however, to his second species, the title of *tuberose.*

Ferrarius. Parkinson. Bauhin. Ray.

Miller.

I meet with nothing more of any consequence respecting it, till Philip Miller, the pride of every British gardener, published the first edition of his Dictionary in 1731. He makes

makes it a distinct genus from *Hyacinthus*, and describes the variety with double flowers, now so common, but then only to be seen in Monsieur de la Court's garden, near Leyden, whose memory is most justly consigned to infamy by our author, for destroying many hundreds of the roots, rather than parting with a single one to any other person; an instance of narrowness of mind and illnature, he adds, too common among the lovers of gardening. I trust no one who belongs to this Society will ever deserve a similar reproach. At this period we find the roots were annually imported into England, along with *orange* trees and *myrtles* from Genoa, and to the directions there given for blowing them, so as to have a succession of flowers from June till October, nothing can be added.

Instance of selfish illnature in a florist.

Imported with orange trees & myrtles from Genoa.

Though our gardens now are enriched with a profusion of other fragrant and beautiful flowers, the *tuberose* still continues to maintain its superiority, and we receive roots, especially of the double variety, from the warmer provinces of North America, as well as Italy. There is no necessity, however, to be indebted to foreign countries for this supply, as I can speak from experience, having cultivated it in the open air for many years at Chapel Allerton, notwithstanding the average temperature of that hill from the month of April to October is far less than in the adjacent valley. If a sufficient degree of heat in summer can only be obtained to bring the leaves out to their full magnitude, that of the roots follows of course, and very little more care than what is bestowed upon the *artichoke*, will preserve them from the severest frosts.

Still much prized.

May be cultivated at home.

For this purpose, select a piece of ground that is perfectly drained, under a south wall; or, if this cannot be spared, defend it on the north by a reed hedge. The size of the bed must be proportioned to the number of roots you want, for the same tuber never blows a second time, but only the lateral ones, which are produced in great abundance round it: as they are to be planted at five inches distance from each other, a bed nine feet long, by three feet wide, will hold 144 roots.

Method of culture.

The soil, in which I have found them succeed best, is light

Soil.

sandy

sandy earth, mixed with a third part of very rotten cow dung: the earth should be taken about seven or eight inches deep, along with the green turf, chopping it very small with the spade, and turning it once a month for a year before it is used; if the earth is not very light, add a quantity of sea sand, or fine shelly gravel. If you are obliged to use this compost sooner, pass it through a wide screen, casting out nothing but any large stones.

Preparation of the bed.

About the middle of April prepare the bed as follows: first, take out all the old earth, to the depth of two feet and a half, or three feet, filling it nearly to the top with fresh stable dung, that has been cast into a heap to heat a fortnight before: lay the dung evenly in the trench, treading each layer very firmly down with a board, under your feet, and reserving the smallest and shortest for the last: upon this lay eighteen inches in depth of the compost, sloping it well towards the south, not only for the benefit of the sun, but to throw off violent rains.

Planting.

In a day or two after, plant your roots at five inches distance from each other, observing to place them alternately in the rows, and that the crown or upper part of the tuber is only just covered with earth. These should be the offsets of such as after flowering the preceding year have been preserved from frost through the winter in sand, as well as the strongest remaining upon any fresh imported ones. Till you obtain a sufficient stock, even the weakest may be planted, but as a great number are annually produced by every root, in time those which are large enough to flower the following year need only be selected.

Management after planted.

Cover the bed at night, especially if frosty, with a double mat, till the leaves appear, but give little or no water, protecting it carefully from heavy rains. When the leaves are about an inch long, add a little fresh compost to the surface, filling up any inequalities, and removing all weeds. If the season prove dry, it will now require watering, and towards the end of June and in July, when the leaves are in full vigour, very copiously; but this must depend upon the weather. From this period till the beginning of winter, nothing more is necessary than to weed the bed, and protect it from the autumnal rains: this may be

be done by sloping the ground more up to it, or if you have a cucumber frame not in use, it may be employed for this purpose, taking care to sink the front so low as to admit all the sun possible. About the first week in December, take the advantage of a dry day, and after clearing away all the decayed leaves, thatch the bed all over, and at the sides, a foot thick with dry straw, sloping it well to throw off the wet.

About the middle of February, if not prevented by severe frost, take up all the roots, preserving their fibres, and pack them in very dry sand, in cellars where the cold cannot penetrate till April, when they must be replanted as before, shortening their fibres more or less, as you find them decayed. If the climate was even milder than ours, I should recommend the roots to be taken out of the ground, and preserved in dry sand, for it throws them into a complete state of rest, and disposes them to form their flower stems earlier. Many offsets will by this time have made their appearance round each root, all of which, except two or three at most of the strongest, should be cut entirely out, and this operation must be in some degree repeated after they are planted and growing, as fresh offsets are produced, for, if permitted to remain, they will rob the other buds of sufficient nourishment.

Roots to be taken up in February, and replanted in April.

This second year some of the largest roots will probably flower: if they send up their stems early it will only be necessary to stick them carefully, when about a foot and a half high, and leave them to blossom in the open air; but when they appear later than July, they should either be removed into pots, with a trowel, preserving all the fibres possible, and placed in a stove, or if you have not that convenience, cut out the flower stem, with all the central leaves, as soon as it is discovered, which will strengthen the offsets. In the succeeding winter thatch the bed, taking up the roots in February, as before, most of which will now be strong enough to flower, and may be selected for sale: such roots, if wanted for early forcing, will have a decided advantage over imported ones, for, as their fibres will not be entirely decayed, they will

Management in the second year.

will push immediately upon being removed into brisk heat, and may be brought to flower as early as May.

Estimate of expense and profit.

According to the abovementioned distances, half a quarter of an acre would contain 15,125 roots, leaving nearly as much space for the alleys as the beds, which, at 3d. each, amounts to the sum of £189 1*s.* 6*d.* and as when a sufficient stock of offsets to select the largest was obtained, the annual return of blowing roots may be estimated at half the number planted, the profits of a bed of *tuberoses,* after deducting every expense of rent, dung, and labour, would be considerable, even if it were necessary to cover it in autumn and winter with three light frames. There are many places in our Island where I should imagine this plant might be cultivated with still less care and attention, especially in the southern counties near the sea; in the vicinity of London. Ham Common, Sunbury, and Walton upon Thames; in the Isle of Wight; about Southampton; below Exeter; Bath and King's Weston; in South Wales: and the theory which I would recommend any intelligent gardener to adopt in its general management is, to keep the roots growing as vigorously as possible from May to October, but in a state of complete rest and drought for the remainder of the year.

Places favourable for it.

General remarks.

IX.

Geological Remarks on a calcareous Mountain near Chessy, in the Department of the Rhone: by Mr. L. F. LEMAITRE, *Inspector General of Gunpowder and Saltpetre.**

All natural facts worthy notice to the geologist.

I Do not think there exists a natural fact, or an obfervation however slight, that does not merit the attention of the geologist, who should study incessantly the voluminous book that Nature has laid open before his eyes. Certain pages of this book it is true may appear but little interesting; yet he should not pass over a single one if possible, would he attain an accurate knowledge of the interesting history of our globe, and the wonderful revolutions it has undergone. A mountain,

* Journal des Mines, No. 106, p. 307.

a mine,

a mine, a quarry, an earthslip, are so many pictures, in which geologists may discern this history.

From the same considerations I am induced to think, that they will be gratified by the account and sketch I here present them, which appear to me to exhibit something singular, if not problematic, which it is for them to solve. See Pl. II, fig. 1.

Vale near Lyons.

The schistose vale, in which the village of Chessy, near Lyons, is built, is bounded on the north-east by a chain of mountains of no great height, which appears to run south-east and north-west, and in which a well known mine of yellow sulphuret of copper is wrought. On the opposite side of the vale is a chain of mountains of two or three hundred yards high, nearly parallel to the former chain, but not stretching so far to the north-west, and cut about three quarters of a mile from Chessy by another vale, meeting the first almost at a right angle.

Calcareous mountains on a base of schist.

Copper mine.

The last mentioned chain is calcareous from its summit about two thirds of its height. Its base appeared to me to be composed of a schistose rock, similar to that which composes probably the first chain and the whole of the intermediate valley, since the vein of copper, which has been wrought to the depth of upward of a hundred and sixty yards, is enchased in this rock.

Limestone quarry at the top.

Strata variously inclined.

The extremity of the high calcareous chain, at the kind of promontory it forms where the two vales meet in an angle, exhibits at its summit a large quarry of calcareous stone, which is used for building in the adjoining country. A perpendicular section eighty or ninety feet high, made in a direction nearly east and west, exhibits a series of strata from eight to fifteen inches thick, not arranged horizontally, but with different degrees of inclination to the horizon, and crossing one another in various directions, as seen in Pl. II, fig. 1, which is from a drawing taken on the spot. The value of the angle I have inserted at the different arrangements of the strata is only estimated by sight, as I was unable to measure it, on account of the steepness of the place. It is according to the decimal division. All the strata in each arrangement are parallel, except those marked A & B, which grow wider, the first as it descends, the second as it follows

Two strata widen in their course.

follows the curvature of the strata on which it rests. Each of these is very distinctly separated from that which immediately follows it, or from the head or base of those it covers, or which abut against it by a kind of saalbande of the same nature as the strata, but of another colour.

Stone coloured by iron, and contains a few shells.

The stone of this quarry has a pretty fine grain, is rendered yellowish by oxide of iron, and contains a few shells. There are found in it small bivalves of the chama kind, crowstones or gryphites, and a few belemnites. The shelly and coarse strata serve for building, the fine and hard strata are used for entablatures and other ornamental parts.

Mine counsellor Gillet l'Aumont has given such a satisfactory explanation of the angles and tortuous bendings of certain veins of coal, and other alluvial strata, such as those of bog iron ore near Sarre-Libre, that he seems to have caught Nature in the very fact. Whether his ingenious hypothesis will account for the arrangement of the strata in the quarry at Chessy, I must leave to his consideration.

X.

Remarks on a singular Arrangement of Strata observed in the Chain of Jura, in the Department of Doubs: by the Same.*

Of what use is the observation of Nature?

OF what consequence is it, some will say, whether the constituent parts of our globe be arranged in this way or that? What signify to us the causes of the regularity or disorder they may exhibit, if the order of Nature as a whole be not disturbed; if every thing in the universe be as it ought to be?

It may be abused,

No doubt the abuse of observation, for every thing has its abuse; no doubt the desire of explaining every thing, not excepting what exceeds the limits of our narrow comprehension; have led natural philosophers into useless researches, and into idle explanations, that frequently betray more vanity, than desire of being useful: but I conceive there are facts in geology, which it is advantageous, I will not say in

but is certainly beneficial.

* Journal des Mines, No. 106, p. 310.

all

Strata in a calcareous Mountain near Chesy.

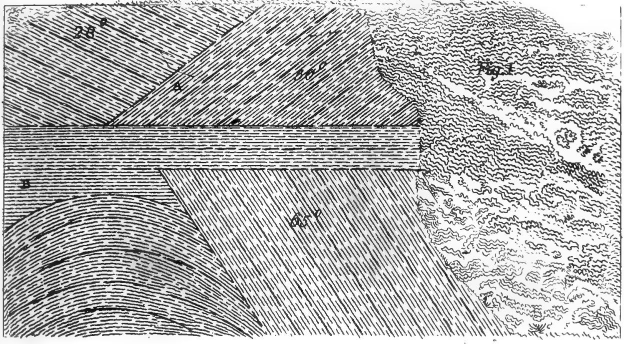


Fig. 2. Strata in the Department of Doubs.

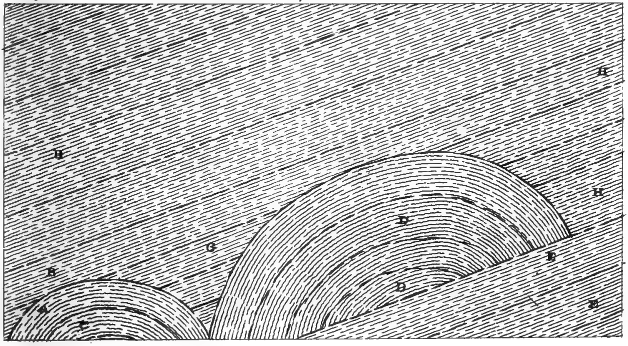
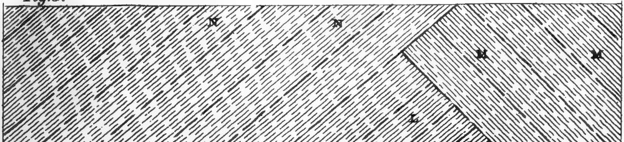


Fig. 3.



all cases to explain, but at least to observe accurately, and to make known, because they are of importance to an art of essential utility, an art founded on observation, that of the miner.

Lead to a knowledge of mines.

The course of the strata of combustible minerals and metallic veins, their various directions and inclinations, their bendings, turns, faults, disappearance, and change of position, their impoverishment, &c.; all these different states, all these modifications, appear to depend on the arrangement of the strata of our globe, and the concussions they may have experienced at different periods, whatever the causes may have been. Perhaps therefore it is not useless, to make known any singularities of this kind, hitherto little observed, that may offer themselves.

Thence useful to the miner.

Since it is from the bosom of the earth we derive the materials of our most useful arts, minerals, to study its internal constitution is of importance, as this would frequently lead to the solution of the difficulties that present themselves to the miner, or at least diminish their number. His progress therefore, being less uncertain, would be much less expensive.

The author's claim to confidence.

If my reasoning be just, I shall bring forward with more confidence a few observations, that appear to me to claim the attention of geologists, from the singularity of the facts. They may confide in what I have the honour to lay before them, since, as I am not sufficiently initiated into natural history to form systems, I have always confined myself to observation, and to observe long before I copy; and in the present instance I have the confirmation of several fellow travellers, among others the senator Aboville, whose accuracy of observation is well known.

Jura.

The table land of Jura, on which stands the city of Pontarlier, is furrowed by a few valleys, more or less close. One of the most interesting is that of la Loue, on account of its wild and picturesque scenery, and the various works constructed on the banks of the river. It is rendered particularly remarkable by the source of the river itself. I do not think the reader will be displeased with my saying something here of this wonder of Jura, as it may be styled, which de-

Vale of la Loue.

serves on many accounts to be visited both by the naturalist and the lover of the arts.

The vale described.

The valley of la Loue begins above the village of Monthier-Haute-Pierre, between Pontarlier and Ornans, in the subprefecture of Pontarlier, in the department of Doubs. This valley, very narrow at its origin, and very deep, and almost perpendicular throughout, exhibits in this part the appearance of a vast well, opened on one side to let the water flow out. Its sides are composed of compact, gray, calcareous rocks, veined with white carbonate of lime in a state of confused crystallization. At the foot of these rocks, but nine or ten yards above the bottom of the valley, a dark cavern, the depth of which is unknown, and the mouth of which is about seventy yards wide and thirty-five yards high, pours out with great noise a very copious torrent of clear water, that tumbles foaming among the rocks, which it has torn off and driven before it. The depth of the valley, the beetling cliffs that form it, the aspect of the cavern, the roar of the torrent rushing out of it, the mist it throws up, the gloom that reigns in this savage place, the bottom of which has never been illumined by the rays of the Sun confined to the tops of the rocks, all conspire to give an idea of the chaotic disorder, that prevailed before human industry had laid earth, water, air, and fire under contribution for the benefit of the arts.

Cavern, from which issues a river.

Number of manufactories established on it.

By regulating the course of these waters, or of part of them; gaining by the explosion of gunpowder a few yards of surface from the adjacent rocks; and by suspending erections over the torrent itself; a number of different manufactories have been established at the foot of this precipice, forming a complete contrast between art and nature. The Loue, after it issues from the cavern, is divided and turned in numberless directions to set in motion eight or ten flour mills, oil mills, mills for bruising hemp, forge bellows, large and small hammers, flatting mills, cylinders for cutting iron into bars, and saw-mills. These wonders, which are daily increasing by the addition of fresh structures, are owing to the industry and activity of Mr. Besson, administrator of saltworks. I had forgotten to mention, that you get to the bottom of this enchanted precipice by a flight of steps, the windings

windings of which, concealing it from your view till you reach it, render the picture more magical, and the surprise the greater.

It was in the cliffs forming the walls of this narrow basin, that I had an opportunity of noticing some singular arrangements of the strata composing it. Every thing exhibits traces of the derangement I had observed in many other parts of Jura, but here they appeared to me larger and more varied than any where else. I cannot give a better idea of them than by the drawings I made on the spot, engravings from which are annexed. See Pl. II, figs. 2 and 3. Strata distorted.

It is to be remembered, that fig. 2 represents the face of the rock to the right of the cavern, the entrance of which commences at a very little distance from the natural vault A; so that the cavern has been opened through the strata B, B, and those resting upon and parallel to them, which dip toward the centre of the mountain at an angle of about thirty degrees. This fact will give an idea of the effect and of the time required for the water, or the acting power whatever it was, thus to force its way through immense strata of a hard and compact rock. Cavern.

The strata A, C, &c. exhibit a semicircular arch; those marked D, D, an elliptical arch. Both appear to rest on the strata E, E, which are no doubt produced to the left under an angle nearly similar to that of the strata B, B; and probably receive the extremity A, C, of the smaller arches. Natural arches.

All the little veins or laminæ, that compose each stratum, have regularly undergone the same curvature; so that the arcades and all the curved parts of the strata of this mountain exhibit the appearance of a book bent in different directions. Laminæ of the strata regularly curved.

I could not get at the knowledge of the arrangement of the part below, F, F, which forms the floor of a sort of yard belonging to the manufactory. Mr. Besson is making an excavation under the natural arch D, D, for the purpose of a storeroom or workshop. Part beneath not visible.

The existence of the portions or rudiments of strata, G, G, G, H, H, H, will perhaps appear singular, but it is not the less real, and is very distinct. If I might be permitted to hazard an opinion respecting them, it would be, Depositions in the angles of the arches.

 that

that, from these depositions in the angles of the arches we must infer, that the arches were in being before these depositions were formed.

Another part of the mountain. Natural arch. Fig. 3 represents another face or precipice of the same mountain contiguous to that represented in fig. 2; so that it forms one contiguous mass, but with an obtuse saliant angle at *a b*. In this part too there is an arch I, of larger dimensions than either of the others, and much flatter. It seems to form a support for the upper strata, L, L, L, which rest on its extrados.

Contorted strata. The singular contortion of the strata K, K, is represented exactly as it is in nature.

Strata intersecting. The strata L, L, L, M, M, M, and N, N, cross and mutually intersect each other, without losing any of their regularity in this part. Though I observed them from a distance, their general and reciprocal arrangement is too conspicuous, for me and my fellow travellers to have been deceived.

Similar appearances in other parts of the mountains; In thus observing the two sides of the mountain analogous dispositions of the strata are observable. I give here the most striking, but all of them merit the attention of the geologist.

and elsewhere. The mountains of Jura however are not the only ones, in which I have noticed phenomena analogous to those I have described. I have had opportunities of observing such in the calcareous mountains of the Lyonnese, in that which overlooks the village of Chessy, seven miles north of Lyons, toward the west, in a quarry on its summit, and of which I lately sent an account and drawing to the council of mines. See the preceding article, and Pl. II, fig. 1.

In coal and other mines we every where find examples of great disturbances happening to the surface of our globe, disturbances that must have occured at periods very remote from each other, and that excite our astonishment. Ages to man are but moments to nature.

XI.

An Inquiry into the Causes of the Decay of Wood, and the Means of preventing it. By C. H. Parry, *M. D.*

(Continued from Vol. XIX, p. 338.)

WHEN wood decays under cover, that condition is usually called the dry-rot. Let us examine the circumstances in which this change takes place. Dry-rot.

It affects the interior doors, shelves, laths which subdivide the layers of wine, and all other wood work in certain cellars; beams and rafters which support the roofs of close passages; joists laying on or near the earth; the wainscoting of large rooms, little inhabited, in old and especially single houses; and wood in various other situations of a similar kind, which need not be particularized. In some of these cases, while one sample or portion of wood shall suffer the dry-rot, another specimen or portion shall remain unchanged. In other instances, wood of various kinds and qualities has been successively employed, and all has alike suffered. During the stages of change, a crop of mucor or mould, and very frequently of fungi, has sprung from the porous mass; and the decay is always attended with a widespreading exhalation, the odour of which cannot well be described, but which is sufficiently known.

Places where it commonly occurs.

Attended with mucor, or fungi, and a peculiar smell.

What then are the causes of this destruction; Precisely the same as those which I have before described; though their action is differently modified, and less obvious to gross observation. The decay is produced by the putrefactive fermentation of the component parts of the wood, in connection with moisture, without which, as I have before stated, wood cannot putrefy.

Cause.

Common air is not only capable of mixing with a considerable quantity of water in form of vapour, but during every state of our atmosphere is always much loaded with it. Water becomes vapour in consequence of being united with a certain proportion of that substance which is called heat.

Air loaded with water,

which is deposited on any thing colder than the air.

heat. If a sufficiently cold substance comes into contact with vapour, the superabundant heat, which was necessary to its existence in that form, passes into that cold substance, and the vapour is then immediately condensed or changed into water. Thus if in the hottest day in summer, when the vapour in our breath is totally invisible, we breathe on a looking-glass or plate of polished metal, which is colder than our breath, the surface is immediately dimmed; and if we continue to breathe on it, small drops of liquid appear, which gradually become larger and larger, and many of them at length uniting, run down the surface in a stream. The same thing takes place on the outside of a glass of water drawn in summer from a deep well, and of a bottle brought up into a warm room out of a cool cellar; and on the inside of our windows in frosty weather. On the other hand, we could not dim with our breath a plate of metal or glass of 100 degrees of heat, which is greater than that of our breath, and no mist is observable on the inside of our windows during the heat of a summer's day; nor is there any condensation of moisture on the outside of a glass of cold water fresh drawn from the well, or of a bottle out of a cellar, when either is brought into the open frosty air.

Many circumstances explained thus.

These circumstances will explain many appearances, by which, for want of due examination, we are often greatly puzzled. We are frequently mortified by seeing in our houses, especially in the country, the walls become stained, or the paper separated and hanging down, and often perishing; and as this usually happens on the side or corner which is most exposed to the weather, we conclude that the damp comes through the wall, and tax our faculties to the utmost, in order to prevent this penetration. The measures which we employ sometimes succeed. But it often happens, that casing, and plastering, and painting the devoted angle fails; and then, as the last resource, we take off the paper and attach it to canvass at the distance of one or more inches from the wall, and thus, for the present at least, effect the desired purpose. Now in this case it is just as absurd to suppose, that the wet comes through the wall, as that it comes through the glass window in a frosty day, or the glass or bottle from the well or cellar. The fact is, that in an exposed

Dampness of certain walls.

The wet does not come through the wall,

posed house, and more especially on the most exposed corner of a room seldom warmed by fire, the inner surface of the wall, by the continuance of frost, is become of a very low temperature, like the air within the room itself. So long as this state of equal temperature between the wall and internal air continues, or if the wall is warmer than that air, it is obvious that the vapour which is mixed with the air cannot part with any heat to the wall, and therefore will not undergo condensation; just as no dampness appears on our windows during a hot day in summer. But if a thaw comes on, and the air becomes warmer than the wall, which, from its capacity of easily shifting place, it will readily do, then the vapour, which is mixed with it, parts with its superabundant heat to the colder wall, and appears on it in moisture or drops, or pours down it in streams; just as happens to the cold bottle brought into the warm dining-room.

but is deposited from the air in the room.

This change is the greater, the more completely the materials of the wall fit it for carrying the heat out of the vapour, or, in philosophical language, the better they conduct heat. Hence a wall painted in oil condenses vapour, or runs with water, sooner than one, which, being unpainted, is more porous; for which reason, in cities, we first perceive dampness and drops or streamlets of water on the oil-painted party walls which bound our staircases, and which are, therefore, absurdly said to sweat, though these walls have no communication with the outward air, and, from their varnished covering, cannot admit of the passage of moisture or perspiration through their pores.

A wall painted with oil soonest wet.

In this case the remedy is obvious, and by its success shows the nature of the evil. Prevent your walls from ever becoming colder than the warmest external air of winter, and you will never have this appearance of damp on their inner surfaces.

Principle of prevention.

This may be done, first, by constructing the walls of such a degree of thickness, or with such a disposition or quality of materials, that they shall not, in the usual way, be greatly cooled throughout their whole substance by any temperature of the outward air. With this view, I think that in all single houses, which are not warmed by neighbouring fires, and more especially in situations exposed to high winds, and therefore

Method of applying it.

Detached houses require thicker or double walls.

therefore to great evaporation from the external surface, and consequent abstraction of heat, the walls should always be double, having on the inside a thin layer of brick, with an interval of one or two inches from the outer and thicker layer of brick or stone, to which it must be united by proper binders. The porous structure of the bricks, added to the impermeableness of the intermediate stratum of air, would so ill conduct heat, that such walls would necessarily tend to keep a house dry and warm in the winter, as well as cool in the summer. This end would be still further promoted by filling the interval between the two layers with dry sand, fresh sifted coal-ashes, or powdered charcoal. In fact, when the common external means before described have succeeded in curing dampness, it has been either by affording a varnish, which has diminished evaporation by preventing absorption, or by increasing the space or changing the quality of the materials of the wall through which the heat was to pass, so as in either of these cases to retain it more forcibly: And when the dampness has been remedied by removing the paper to some distance from the wall by means of strained canvas, that effect has been produced by rendering the paper a worse conductor of heat; and therefore indisposing it to condense the vapour in the room so readily as when it was in contact with the colder wall.

How the common methods sometimes succeed.

Not possible to keep out the cold.

It has been suggested, that it would be possible to keep out cold, or, in more accurate language, prevent the egress of heat from the inside of a room, and therefore from the walls surrounding it, by shutting it closely up, and preventing any admission of the cold external air. This has arisen from the supposition that air is not a good conductor or transmitter of heat through its substance or pores, but that it merely carries it by changing place with some other portion which was less charged with it. If there were no other mode of abstracting the heat from the walls of a room, and if it were possible wholly to prevent any change of its air, this theory might perhaps apply. But it is not possible to prevent some exchange of this kind through the atmosphere of any habitable chamber; and it is evident from the moisture being most abundantly, or perhaps solely, deposited on the inside of that part of the wall which is most exposed to the

Not wholly,

the external cold, that the chief or common mode in which the wall is cooled is not by the access of the cold air into the room, but by the passage of heat from the wall itself into the cold air without. We may however so far avail ourselves of this principle, as to exclude as much cold air as we can, by shutting up the windows and chimnies of uninhabited rooms during the severity of frost.

but in part.

It may farther be suggested, that as, during a thaw, the air, being warmer than in frost, has a greater quantity of water in form of vapour mixed with it, shutting up a room on such occasions may, by retarding the admission of warmer air so charged with vapour, allow time for the walls to acquire an equable temperature through their substance from without, so as to anticipate any condensation on their surface which might occur from the free admission of the external air. To this I only answer, as before, that rooms according to the common construction cannot be excluded from communication with the external air; and that, in fact, the dampness does under these circumstances take place, though the doors and windows are never opened.

Shutting up a room when it thaws not sufficient.

In all cases, however, there is one method of preventing this species of dampness, which is infallible; and that is to keep every part of the internal surface of the wall in the chamber or staircase sufficiently warm by good fires. With this view all staircases ought to have some means of receiving artificial warmth.

The wall should be kept sufficiently warm by fire.

If, notwithstanding this and the former precaution, a wall should accidentally become damp, the next best expedient is to dry it as quickly as possible by a free current of warm air.

Dried if necessary.

This discussion, which at first sight might appear tedious and irrelevant, will, I trust, no longer be thought so, when it shall have been found necessary for the establishment of a principle on the subject more immediately before us.

In order to show the analogy, let us take the simplest example, which is that of a wainscotted room, unwarmed by fires. When the wainscot is colder than the air, it condenses the vapour in form of moisture. If that moisture were exposed to the influence of the sun and wind, the case would come under the former head of decay, which is that of

Analogy in the case of dry-rot.

of wood wetted by rain in the open air. The water soon evaporates, and little decay proceeds in the wood. So in the wainscot, the surface next the room, though unprotected by paint, will perhaps be long in rotting, because the room admits of currents of air, more especially when doors and windows are frequently opened, so as to evaporate the superficial moisture, though less quickly and effectually than in the open air. But what is the case with the surface of the pannel next the wall? The air, loaded with moisture, penetrates into that interstitial space, and deposits it by condensation on that surface. But there is afterward no current of air to evaporate the water so deposited, which then slowly decomposes and destroys that surface of the pannel. Such is precisely the process of the dry rot, which always begins next the wall, and gradually proceeds to the painted or outer surface of the wood. It resembles in its chief circumstances the decay of paper in a damp room; and it precisely resembles that of paper projecting from the wall on canvas, which will still often happen, if the wall be subject to acquire a very considerable degree of coldness, though much more slowly than in the former case.

In wainscot.

Other cases similar.

The same process obtains in all other cases. Whenever the wood is cooler than the air which it touches, the vapour is condensed upon it; and being exposed to no new heat or current of air sufficient again to evaporate it, remains till another fit of condensation affords a new supply.

Thus the process of corrosion and decomposition is continually supported, till the wood moulders away.

Dry-rot an improper term.

The term dry-rot is, therefore, so far from being expressive of the real fact, that decay proceeds under these circumstances more quickly than in the open air, precisely because the wood is more constantly and uniformly wet; just as the lower parts of posts and rails, and any cavities in timber exposed to the weather, rot sooner than those parts which readily and speedily dry.

Cause of the smell.

The smell which we perceive on going into vaults or cellars, where this process is going on, arises partly from the extrication of certain gases, mingled perhaps with some volatile oil, and partly from the effluvia of those vegetable substances, which have already been said to grow on it; and which,

which, though they begin merely because the decayed wood is their proper soil, yet afterward tend probably to the more speedy decomposition of the wood itself. They cannot, however, with more propriety be said to be the cause of the dry-rot, than the white clover, which appears on certain lands after a top-dressing of coal-ashes, can be said to have produced the soil on which it flourished.

Some have ascribed the dry rot to the nature of the wood.

I have remarked above, that sometimes only a particular sort or sample of timber has in certain situations rotted, while another piece has continued for a great length of time perfectly sound. Hence persons have been deceived, and been disposed to attribute the dry-rot solely and universally to some original peculiarity in the wood itself. Dr. Darwin explains this fact by telling us, that the wood so decaying has probably been cut in the spring, when the sap in the alburnum was not only abundant, but of a saccharine quality; which, in combination with the vegeto-animal substance or gluten, disposes it to run with unusual readiness into destructive fermentation. In some trees, as by more particular custom the oak, the bark is a very valuable article of commerce, and is found not only to quit the tree more readily, but to contain a larger proportion of tan in the spring, when the sap is rising, than at other seasons. Hence an old Act of Parliament, now in force, ordains that all oak, except for the purpose of building, shall be felled in the spring. Whether doors, posts and rails, paling, barrel staves, &c., come under the denomination of building, it may be difficult to say; but it seems at first view highly to be lamented, that any law should impose an obligation to destroy a valuable species of property. It would indeed be matter of peculiar regret, if an impolitic and avaricious spirit should induce the owners of oak forests to extend the same principle to the timber employed in the construction of great machines, and more especially the British navy.

Wood felled in the spring liable to it.

Means employed to remedy this.

Various means have been employed in order to remove the tendency to the dry-rot in trees so felled. Thus they have been long exposed to the rain, or steeped, or even sometimes boiled in water, and then dried by artificial heat. These means do not however appear to have been successful

in

in entirely washing out the fermentible sap, which therefore makes them much more subject to the decay of which we are treating. It may however still be doubted, whether it acts in any other way than by furnishing a disposition, which requires to be called into action by the same cause which operates in all other cases, moisture.

Instance of their inefficacy.

In proof of what I have stated, I have been informed by one of our Vice Presidents, that in a large vat or set of vats for beer, belonging to him, the staves formed of oak $2\frac{1}{2}$ inches thick, notwithstanding they were previously steeped in hot water, and then thoroughly dried, in a very short time underwent the dry rot, while others in the same situation continued unchanged five or ten times that period. It is highly worthy of remark, that the outside of these staves, which was painted, continued sound, and that the decay began on the inside, where, from the vats being at different times more or less filled, they were subject to the joint and successive influence of moisture and air.

Range of temperature in which it occurs.

I have mentioned above, that the putrefactive fermentation cannot take place except in certain temperatures, the lowest of which, according to Thomson, must be but little below 45 degrees of Fahrenheit's thermometer, and the highest within the degree which produces dryness by evaporation. The temperature most conducive to this effect has not, so far as I know, been ascertained, though much useful information on this head might be obtained from a set of well conducted experiments.

Theory of the dry-rot.

The following then appears to be the whole theory of the dry-rot; that it is a more or less rapid decomposition of the substance of wood, from moisture deposited on it by condensation, to the action of which it is more disposed in certain situations than in others; and that this moisture operates most quickly on wood which most abounds with the saccharine or fermentible principles of the sap. Let us see how this theory corresponds with the best known means of prevention, and what more effectual measures it may suggest.

Timber should be felled at a proper time, & well dried.

The first point is certainly to choose timber properly felled and well dried. And here, in order to prevent the injudicious fall of large oak timber, it may be of some consequence

sequence to know, that the bark of such timber contains much less tan than that of the younger and more succulent wood; and that this principle, together with the proper extractive matter, is considerably more abundant in the bark of the Leicester or Huntingdon willow, than in that of any oak. According to the experiments of Mr. Davy, $7\frac{1}{2}$ lbs. of the former will go as far in tanning leather as 9 or 10 lbs. of the latter. It has however been asserted, that if an oak, or any other tree, which is stripped of its bark, be suffered to stand two or three years before it is felled, the wood will have acquired a very great degree of strength and durability.

Bark.

Next, where it is practicable, a current of air should be frequently made to pass along the surface of the wood. This expedient seems to have been particularly attended to by the ingenious architects of our Gothic churches, who are said with that view to have left various openings in the walls between the two roofs of those edifices. In order also to promote evaporation, a certain degree of heat, such as that of air heated by the sun or fire, should, if possible, be from time to time applied. Cellars themselves ought to have some communication with the outward air by means of windows and shutters, or trap-doors. And that these may be for a short time opened in proper weather, so as to have a draught of air; and that no very low degree of temperature is necessary for the preservation of fermented liquors, provided that temperature be uniform, is evident from the practicability of keeping wine extremely well in cellars which are not damp, and in which, therefore, one or both of these circumstances must have taken place.

The wood should be exposed to a current of air.

Cellars.

The destruction of wainscotting may be long deferred by keeping in the apartment suitable fires.

Lastly, the dry-rot may in all cases be infallibly prevented where it is practicable to cover the surface of the wood, properly dried, with a varnish which is impenetrable and indestructible by water. With this view two or three coats of the composition before described should be laid on the dry wood, before it is erected or put together, and a third or fourth after it is put in its place; and proper means should be taken thoroughly to dry each successive coat of varnish.

When the wood is dried, it should be covered with varnish.

In

In situations of this kind, what means of preservation are necessary must be employed at first; as it seems scarcely possible to renew them on fixed timber with any chance of benefit.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society, Professor Jameson read an account of a method of constructing and colouring mineralogical maps. We cannot give a satisfactory account of this paper without drawings; we shall therefore only observe, that maps executed according to this plan show distinctly the figure of the cliffs, terraces, mountain ranges and mountain groupes: and the colouring affords a true and harmonious representation of the alternation, extent, and relative position of the different rocks that appear at the surface. Professor Jameson at the same time laid before the society a series of mineralogical queries, which he had drawn up with the view of directing the attention of mineralogists to the particular objects pointed out by them.

We have permission to communicate these queries to the public.

Mineralogical Queries.

ENGLAND.

Mineralogical queries.

1. Does the granite of Cornwall belong to the oldest or newest granite formation, or do both formations occur in that county?

2. Is the schorl rock of Cornwall disposed in an unconformable and overlying position in regard to the older primitive rocks; if this be its position, on what rock or rocks does it rest, and what are its other geognostic relations?

3. Does the serpentine of Cornwall belong to the first or second serpentine formation, and what are the imbedded and venigenous fossils it contains?

4. What are the characters of the different metallifarous venigenous formations in Cornwall: are any of them identical

tical with those described by Werner*, Mohs †, Friesleben ‡, Jameson §, and others? Mineralogical queries.

5. Do the inclined slaty strata in the vicinity of Plymouth belong to the transition class of rocks?

6. Does the upper part of the mountain of Cader Idris in Wales belong to the newest flœtz trap formation?

7. Are not the mountains in Cumberland principally composed of transition rocks partially covered with the newest flœtz trap formation?

8. Is not the porphyry of Cumberland a variety of clinkstone porphyry?

9. Does the gypsum of Cumberland belong to the first or second flœtz gypsum formation?

SCOTLAND.

1. Does the sienitic greenstone of Fassnet burn in East Lothian belong to the transition rocks, or the newest flœtz trap formations?

2. Does clay stone occur in beds or veins in the coal fields of the Lothians?

3. What are the geognostic characters and relations of the porphyritic rock of the Ochil hills?

4. Is Inch Keith in the Firth of Forth entirely composed of rocks belonging to the independent coal formation?

5. Are the geognostic relations of the porphyry slate or clinkstone porphyry of East Lothian the same as in other countries?

6. What are the geognostic relations of the claystone, compact feldtspar, and striped jasper of the Pentland Hills?

7. What is the extent and mode of distribution of the sienite of Galloway?

8. Does the Craig of Ailsa in the Firth of Clyde and the Bass rock in the Firth of Forth belong to the newest flœtz trap formation?

* Neue Theorie von der Entstehung der Gänge von A. G. Werner, 1791.

† Beschreibung des Gruben-gebäudes Himmelsfürst. von F. Mohs, 1804.

‡ Mineralog. Bemerkungen bei gelegenheit einer Reise durch den merkwurdigsten Theil des Harzgebirges, von Friesleben, 1795.

§ Mineralogical Description of Dumfrieshire, 1805. Elements of Geognosy, 1808.

9. Does

Mineralogical queries.

9. Does the pitchstone of Ardnamurchan belong to the newest flœtz trap formation?

10. Is the granular quartz in the islands of Isla and Jura subordinate to mica slate, or does it constitute a distinct formation?

11. Are the Cullin mountains in the isle of Skye composed of rocks belonging to the newest flœtz trap and second porphyry formations?

12. What are the geognostic characters and relations of the obscure egg in the isle of Egg one of the Hebrides?

13. Of what rock is the isle of Staffa composed, and what its geognostic characters and relations?

14. Is the porphyry of the isle of Rasay porphyry slate?

15. What are the geognostic relations of the tremolite of Glen-Elg in Invernesshire?

16. Does the upper part of Ben Nevis belong to the second porphyry formation; and if this be the case on what does the porphyry rest?

17. Does the porphyry of the Brauer near Blair in Athol belong to the first or second porphyry formation?

18. Does the granitic rock in the vicinity of Aberdeen belong to the granite or sienite formation?

19. Does the sandstone of the Shetland islands belong to the independent coal formation, or to any of the formations described by Werner?

20. In what species of mineral repository are the ores of Sandlodge in Shetland contained, and what are the oryctognostic and geognostic characters and relations of these ores?

21. Does the claystone of Papa Stour, one of the Shetlands, belong to the newest flœtz trap, or coal formations?

22. Does the serpentine of the islands of Unst and Fetlar belong to the first or second serpentine formations?

TO CORRESPONDENTS.

F. R. S. will perceive that the communication from Professor Vince, inserted in our Supplement, renders it less necessary to insert his favour. At the same time that his general remarks upon the spirit most desirable to be shown in controversional writings must be allowed, it must be admitted in behalf of the Editor of a periodical publication, that very cogent and manifest reasons ought to present themselves, before he can be justified, for interfering in the discussions transmitted to the Public.

The Editor having been, contrary to expectation, disappointed of the Meteorological Register, is still obliged to postpone it: but he will take proper measures to prevent farther delay.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1808.

ARTICLE I.

Observations on such Luminous Phenomena in the Atmosphere, as appear to depend on Electricity. By a Correspondent, (R. B.)

IT has been long since incontrovertibly established, that lightning is the electrical stroke between the clouds and the Earth, or between one cloud and another. All the differences of opinion therefore relate at present to its attributes or affections, which philosophers have not scrupled to investigate by the assistance of the electrical machine. But there are many circumstances, for an explanation of which we must have recourse to the great theatre of nature.

Lightning is electricity.

The luminous appearances seen above the surface of the earth are, ignes fatui, lightning, shooting stars, fire balls, and the aurora borealis. Whether the first be an electrical phenomenon has not yet been satisfactorily ascertained, and indeed their cause may be said to be entirely unknown; but lightning and the aurora borealis are perfectly imitable by electricity; and it is highly probable, that an electric spark would exhibit the appearance of shooting stars and fire balls, if of sufficient length and remoteness to permit its figure and angular velocity to be perceived. It is also probable, that

Enumeration of luminous meteors.

The electric spark or fire ball.

that the electric explosion consists of a ball or cylinder of no great length, ignited by the compression of the air or gas, or other fluid it drives before it. Admitting this, the zigzag spark with ramifications may be considered as a fire ball continually throwing out detached pieces; the brush will be a fire ball broken to pieces, and the lightning will not differ from fire balls but in its vicinity to the Earth, and its velocity, which is perhaps greater. An artificial fire ball moving slowly has been seen once, and but once, by Warltire the lecturer. See Priestley's Electricity.

The velocity of disengaged electricity 23 miles in a second for lightning,

The magnificent experiments of Watson on Shooter's-hill, in which the shock was transmitted through great lengths of wire, teach us nothing of the velocity of disengaged electricity, as there is no proof that it has any known relation to that of the electric matter passing through conductors. Most persons think they can distinguish the direction of lightning, but this may perhaps be a deception. M. Marat* is the only philosopher that I know of, who has made any observation from which an inference of the velocity of lightning may be deduced; and he himself remarks, that it is attended with various causes of uncertainty. He measured the angular distance between two clouds, from one of which a horizontal flash of lightning flew to the other, and found it 30 degrees: the time was 20 thirds, and the distance determined from the interval of time between the flash and the report, was 10,000 toises. From these data he infers, that the velocity was 19,200 toises per second, which is somewhat more than 23 English miles.

and also for a fire ball;

but it is not likely to be constant.

This determination, by its remarkable coincidence with that of Sir Charles Blagden, respecting the velocity of fire balls, might lead to a conclusion, that there is a settled velocity for luminous electric matter, if it were not credibly ascertained, that it sometimes moves much slower, and is even nearly stationary, according to circumstances. In the storm which happened at Steeple Ashton†, on the 20th of June, 1772, two gentlemen being sitting in a parlour at the vicarage-house, and conversing about a loud clap of thun-

* Marat, Recherches physiques sur l'Electricité, p. 226.

† Ph. Trans. vol. 63, p. 232.

der

der that had just happened, they saw on a sudden a ball of fire between them, at about a foot distance from one of them. They described it to have been about the size of a sixpenny loaf, and surrounded with dark smoke; that it burst with an exceeding loud noise, like the firing of many cannon at once; and that they perceived a disagreeable smell, resembling that of sulphur, vitriol, and that of many other minerals in fusion. One of them was exceedingly hurt. As soon as he was struck he sunk in his chair, but was not stunned; his face was blackened, and his features distorted; his body was burned in several places, small holes were made in his clothes, and he lost in some measure the use of his legs for two or three days. He is positive he saw the ball of fire in the room for a second or two after he was struck. He also saw after the explosion a great quantity of fire of different colours, vibrating backwards and forwards in the room, with a most extraordinary swift motion. This might perhaps be an affection of his sight.

A fire ball in a room

Mr. Field, a painter of Trowbridge, during the storm, observed a ball of fire vibrate backwards and forwards over some part of Steeple Ashton, and at last dart down perpendicularly. This was in all probability the same ball as was seen to burst in the parlour of the vicarage-house.

seen before its descent.

A body of fire was also seen during the same storm moving towards a house, at some distance from the house of Mr. Paradise, which changed its direction and passed through the last house, and afterwards burst with a prodigious explosion. Mr. Paradise, who was three or four feet out of its line of motion, was struck against the wall, his body covered with fire, and he thought for some time he should have been suffocated with the smoke and smell of sulphur. He escaped unhurt, and his house received no damage.

Another in the same storm.

To these instances of electric matter which produced the effect of lightning, though its velocity was too small to prevent its figure being perceived, may be added, the very severe stroke of lightning, which killed two of the servants of Mr. Adair, at Eastbourne* in Sussex, threw himself hurt

Lightning at Eastbourne, of which the figure was observable.

* Ph. Trans. vol. 71, p. 42.

and motionless on the floor, and rendered a young lady and her servant insensible for a time, though these persons were in different apartments of the house, and left considerable marks of its violence on the house and furniture. It happened on the 17th of September, 1780. The morning was very stormy, with rain, thunder, and lightning; and just at nine o'clock a horrid black cloud appeared, out of which Mr. Adair saw several balls of fire drop into the sea successively, as he was approaching a one pair of stairs window; very soon after which, he was struck by a most violent flash of lightning, the effects of which may be particularly seen by consulting the original account. But what more especially applies to the present purpose is, that multitudes on the seashore before the house saw the meteor dart in a right line over their heads, and break against the front of the house in different directions; and all agreed, that the form and flame exactly resembled an immense sky rocket.

Distinction between lightning and the aurora borealis.

These facts show the near resemblance between lightning and fire balls. It is probable however, that the electric matter, when it passes violently through the lower regions of the atmosphere, usually has the form of a spark; that is to say, it passes with an extreme angular velocity in some definite direction. But the masses of luminous matter, which pass along the superior and more rarified parts of the air, appear either in the form of those flashes, which we produce by passing electricity through a vacuum, or in the form of balls of fire. In either case the phenomena are on a scale of astonishing magnitude.

Shooting stars, aurora borealis, and fire balls, are greatly elevated.

Shooting stars, the aurora borealis, and fire balls, have in general been found by the best observations to be greatly elevated in the atmosphere; and indeed, beyond the region where the action of the sun's rays on the air occasions the twilight. Mr. Brydone* frequently observed shooting stars from the mountain St. Bernard, one of the high Alps, and also saw several from the highest region of Mount Etna, and they always appeared as high as when seen from the lowest grounds. I find however one curious instance of lights resembling both the aurora borealis and shooting stars, at a much lower elevation.

* Ph. Trans. vol. 63, p. 167.

As

As Mr. Nicholson *, teacher of the mathematics at Wakefield in Yorkshire, was returning on horseback on the 1st of March, 1774, from Crofton, a village near Wakefield, he saw a storm approaching in the north-west quarter, from which the wind sat. It was then about half past six in the evening, and the weather was so dark and overcast, that it was with difficulty he could find his way. When the storm began, he was agreeably surprised to observe a flame of light dancing on each ear of his horse, and several others on the end of his stick, which had a brass ferule notched with using. These appearances continued till he took shelter in a turnpike-house.

Appearances in a storm resembling the aurora borealis

After having continued about twenty minutes the storm abated, and the clouds divided, leaving the northern region very clear; except, that about ten degrees high there was a thick cloud, which seemed to throw out large and exceedingly beautiful streams of light, resembling an aurora borealis, towards another cloud that was passing over it; and every now and then there appeared to fall to it such meteors as are called falling stars. These appearances continued till he came to Wakefield, but no thunder was heard.

& falling stars.

Do not these show two fluids like my sparks?

About nine o'clock a large ball of fire passed under the zenith, towards the south-east part of the horizon; and Mr. Nicholson was informed, that a light was observed on the weathercock of Wakefield spire, which is about 240 feet high, all the time the storm continued.

The present state of our knowledge respecting fire balls, with observations, is exhibited in an excellent treatise written by Dr. Blagden †, now Sir Charles, on occasion of the fiery meteors which were seen in the year 1783. The great meteor of Aug. 18, in that year, had the appearance of a luminous ball, which rose in the N. N. W. nearly round, became elliptical, and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change, compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail, or leaving a train

Treatise of Sir Charles Blagden.

Great fire ball of 1783.

* Ph. Trans. vol. 64, p. 351.

† Ibid, vol. 74, p. 201.

behind.

behind. Under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to the southward, it gradually descended, and at length was lost out of sight. The time of its appearance was 9h. 16m. P. M. mean time of the meridian of London, and it continued visible about half a minute.

Its height 57 miles; velocity 20 miles per second; diameter half a mile; course 1200 miles.

It seems probable, that the meteor burst and united again several times during its course; and that the great change corresponded with the period at which it suffered a deviation in its course. Its appearance was not uniformly bright, but consisted of livid and dull parts, which were perpetually changing their relative position. Its height deduced by computation from the angular elevations from various places, proves much more correspondent than might be expected from such data. One combination gives the height $54\frac{1}{2}$ statute miles, two give 57 miles, two 58, one 59, and one 60: the mean is $57\frac{1}{2}$ miles. It does not appear to have really approached the Earth in its course, which was above 1200 miles in length. Its absolute diameter across, supposing it to have been about half a degree broad, was half a mile, and its velocity was at least 20 miles in a second. A report was heard after its disappearance; and it is very remarkable, considering the rarity of the air at such a height, that the height of the meteor, deduced from the time of the passage of the sound *, nearly agrees with the geometrical deduction: it is $56\frac{1}{2}$ miles. A hissing, whizzing, or cracking, was also said to have been heard during its passage.

Sir Charles ascribes these appearances to electricity. The velocity greatly exceeds that of planetary projection.

After describing the phenomena of the smaller meteor, which appeared on the 4th of October in the same year, Sir Charles proceeds to consider the cause of these phenomena. He shows the insufficiency of Halley's hypothesis, that they consist of a train of combustible vapours set on fire; and also of that which supposes they are terrestrial comets. This last position he observes is incompatible with their general appearance, which does not resemble solid bodies; with their exceeding great number, which could scarce-

* Ph. Trans. vol. 74, p. 111.

ly

ly fail to produce some other appearances, beside a transient illumination; and more particularly with the extreme velocity of the meteor of Aug. 18, which is three times as great as a body falling from infinite space towards the Earth would have acquired, when it came within 50 miles of the Earth's surface. He therefore recurs to electricity, the only agent in nature with which we are acquainted, that seems capable of producing such phenomena. Its extreme and hitherto unmeasured velocity, the electric phenomena attending fire balls, the hissing noise, their connection with and similarity to the northern lights, which have sometimes assumed this form, and particularly their course, which is for the most part nearly in the magnetic meridian, are among the circumstances which are pointed out and elucidated in a perspicuous and highly interesting manner. And he concludes by observing, that if the conjectures he offers be just, there are distinct regions allotted for the electrical phenomena of our atmosphere. Here below we have thunder and lightning, from the unequal distribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling stars; till beyond the limits of our crepuscular atmosphere, the fluid is put into motion in sufficient masses to hold a determined course, and exhibit the different appearances of what we call fire balls; and probably at a still greater elevation above the earth, the electricity accumulates in a lighter less condensed form, to produce the wonderfully diversified streams and coruscations of the aurora borealis. Conclusion.

There is a fact observed by Mr. de Saussure, which seems difficult to be accounted for by the help of our present knowledge of electricity. He was on the Alps with some friends, while a thunder storm formed in the air beneath them. While it lightened and thundered below, they found themselves electrified, but differently, so that they drew sparks from each other *.

I shall finish this communication by a remark of Mr. Winn on the aurora borealis †, that this phenomenon is A south wind follows the aurora borealis.

* Memoirs of the Academy of Sciences for 1773.

† Ph. Trans. vol. 64, p. 128.

usually

usually followed by hard southerly winds, with hazy weather or small rain; which Dr. Franklin, admitting the fact, supposes to be a consequence of the clearness to the northward, which renders them visible, and may have been produced by long continued winds from that quarter; for when the winds have continued long in one quarter, the return is often violent. The later discoveries respecting ignited stones which have fallen from the atmosphere, seem also to belong to the subject of this paper; but I cannot at this time consistently with brevity enter upon them.

II.

Account of the Draining of the Pond of Citis.*

Draining ponds and marshes always considered a difficulty.

THE draining of ponds and marshes has always been considered as a difficult enterprise; and it has frequently happened, that works begun for the purpose have been relinquished, before the object was attained, either because the local circumstances have occasioned too many obstacles, the means employed have been inadequate, or the capital employed has fallen short, before the expected benefit could be derived from the undertaking.

A successful instance given as an example

To instruct and encourage the speculator, as far as is in our power, and enable him to furnish agriculture with new land for the plough or the sithe, we hasten to publish the particulars of the draining of the pond of Citis, which is now going on. We shall point out the difficulties, that have been surmounted; and the new mechanical means, that have been employed.

Description of the pond.

The pond of Citis is to the south-west of the department of the Mouths of the Rhone, at a short distance from an arm of the sea called the Pond of Berre. It is near the ponds of Lavalduc, Pourra, Rassuen, &c. The different quality of the waters of these ponds, and the dissimilarity of their levels, show, that they have no subterranean communication with each

* Journal des Mines, No. 116, p. 137.

each other, though they are so near. The pond of Citis is several feet lower than any of those here mentioned, and, what is very remarkable, it is near twenty-seven feet, English measure, below the level of the sea. This pond may be considered as a spacious basin, enclosed by lofty mountains, in which the rain water has accumulated and become stagnant, having no outlet.

Its level 27 feet below the sea.

The waters of Lavalduc are saline to sixteen degrees*. The proximity of this pond to that of Citis; the facility with which its water might be let into it, by opening a passage through the mountain separating them; and the decrease of the water of Citis after several years of drought, gave rise to the salt-works of Citis. These were undertaken by a company, who subscribed a joint stock to defray the expense. Their plan was to prevent the addition of more water, and gradually dry up the pond, by stopping on the sides of the mountains the course of the rain water, which was its sole supply. This attempt succeeded completely, and the affairs of the company were in a very prosperous way, when, after a memorable winter, the pond was completely inundated by the excessive rains, that fell for three months successively. The company indeed might blame themselves for this disaster; since by their negligence in not keeping the canal in repair, or rectifying its level, the rain-water, being so much more abundant than usual, could not flow with sufficient freedom through it; and thus by its weight breaking down the feeble dike that supported it along the sides of the mountains, it ran into the pond.

Salt-works established there.

These inundated.

This event, of which apprehensions had always been entertained, appeared to admit of no remedy to the company, who had long foreseen, that, if the pond should come to fill at any time, there would be no way to preserve the salt-works, but by carrying off the water over the hills between the pond and the sea. But what means could effect this? There appeared none but the common pump, or the screw of Archimedes; and these being too expensive or inadequate, the company was about to give up the work, when Mr. Augustus de Jessé proposed to drain it by employing a steam engine. Being admitted

Apparently a hopeless case.

Proposed to be drained by a steam engine, forcing the

* This I believe implies, that they contain 16 per cent of salt. Tr.

water over a hill 172 feet high.

ted to present a statement of his design, he showed the practicability of conveying the water into the sea over the hills, though their tops were 172 feet above the bottom of the pond; and that, by adapting the power of the machine to the quantity of water to be raised, he could engage to accomplish it in a very short time. Lastly, as the company seemed undetermined, he agreed to undertake it at his own expense. His proposals and his conditions were accepted.

This might have been effected by a succession of steam engines;

but a single one preferred.

Mr. Jessé might have accomplished his purpose, by placing several steam engines on the ascent of the first hill; the water raised by the first being raised higher by the second, and so on successively, till it reached the top. The power of these engines, which may be increased to any extent, assured him of a given quantity of water in a given time; but such a complication would have been detrimental to the general effect, for the draining could not have gone on regularly, unless all the engines had worked with constant uniformity, which could not easily have been effected. That he might have no obstacles of this kind, and no stoppage, he conceived the design, and carried it into execution, of throwing the water from the pond to the top of the first hill in a single stream, and by means of a single engine. This was adding to the difficulty; but in this the chief merit of the undertaking consists. We shall give an account of the works, by which this was accomplished: and we apprehend the reader will be gratified by the view of them given in Plates III and IV.

The canal for carrying off the rain water first repaired and improved.

After having corrected the errours committed in the construction of the original canal, or drain for the rain-water, carried round the mountains, and encircling the pond, he raised its level considerably, so as to give it a greater descent toward the end where it discharged itself. This canal was supported in the steepest parts by stone causeways; and to prevent the fall of the water into it from being too forcible, he diverted it as much as possible from a perpendicular direction, giving it different inclinations, according to local circumstances.

A well sunk, with two pumps, worked alternately by

At some distance from the pond, on the slope of the hill, the steam engine is erected. A well is there sunk to a level below that of the bottom of the pond, and from its bottom a horizontal

horizontal gallery is carried to the pond at the distance of 320 feet. This gallery, or rather aqueduct, conveys the water from the pond into the well. For this purpose it was necessary, to arch it over completely. In the well are two pumps, and close to it is the steam engine, which works them both alternately by means of a double crank. Adjoining the pumps in the well are two vertical pipes, communicating with them, and united at the mouth of the well by means of an elbow, or fork. The part where they unite is fitted to a cast iron cylinder, 450 feet long, carried up the slope of the hill. This hill not being so high as some of the following, it was necessary to raise the cylinder upon supports of mason work to form a common level. A wooden trough, supported by tressels, unites the first hill to the second. This is 895 feet long. At the end of this trough begins a canal of 2494 feet, which is cut in the rock to the mean depth of 9½ feet. To unite the summits of all these hills it has been necessary to erect several aqueduct bridges, over which the canal is conveyed. The canal might have been cut to less depth, by raising higher the cast iron cylinder, and consequently the wooden trough; but the wind already has sufficient hold of both these, and they could not fail to have been weakened, had they been raised higher. If the iron cylinder had been made to rest on the hill, in order to dispense with the wooden trough, the canal must have been cut to an extraordinary depth, or a gallery of 2500 feet must have been cut through the rock, which would have occasioned an enormous expense.

a steam engine, and forcing the water through a cylinder 450 feet long to the top of the hill.

Thence conveyed by a wooden trough 895 feet long to the next hill; and by a canal with occasional aqueduct bridges to the sea.

The steam of the engine acts upon the pumps, which draw up the water of the well, and force it into the vertical pipes. These convey the water to the ascending cylinder, in which it rises gradually to the top of the first hill, whence it flows through the trough into the canal, which discharges it into the sea.

The water contained in the cylinder acts with all its weight on the valve, that separates it from the fork of the two pipes: yet such is the power of the engine, that at every stroke, of which it makes thirty-two in a minute, it not only raises a certain quantity of water into the vertical pipes, but gives it a pres-

The engine gives 32 strokes in a minute, raising 4660 lbs. of water in the cylinder.

a pressure capable of raising the whole of the water in the cylinder, which is of the weight of 4660 lbs. avoirdupois.

Raises 69611 cubic feet, or 1942 tuns of water in a day.

The engine is calculated to raise 69611 cubic feet of water every twenty-four hours, making a weight of 38845 cwt. or $1942\frac{1}{4}$ tuns. It is obvious, that if it were required to raise a greater quantity of water, and at the same time to a greater height, as of 500 feet for instance, the same steps should be adopted, increasing proportionally the diameter of the cylinder of the steam engine, the dimensions of which give the measure of the power, and increasing the thickness of the cast iron pipe, so that it might be able to resist the pressure of the water forced into it.

Adequate means would produce a greater effect.

Novelties of the mode.

Before the draining of the pond of Citis, we do not believe a steam engine has been employed for such a purpose; still less pumps moved by the usual agents; or that any attempt has been made to raise a large quantity of water to a considerable height in a constant and uninterrupted stream. For this new application of it therefore we are indebted to Mr. de Jessé, and we trust that many enterprising persons will avail themselves of it. In the south of France, and near the coasts of the Mediterranean, there are a great many ponds, which it would be of importance to drain; their vicinity being a scourge to a country in other respects so much favoured by nature. Some attempts that have been made in the departments of the Aude and Gard enable us to presume, that the nature of the soil is in general excellent.

Places where it might be applied with advantage.

We conceive, that no draining can be attended with more difficulties than that of the pond of Citis; that Mr. de Jessé's method is applicable to any pond to be drained, attention being paid to local circumstances; and that is equally applicable to great morasses, the whole produce of which it would be so advantageous to obtain, at a time when the scarcity of fire-wood creates anxiety for the means of supplying the want of fuel.

Explanation of Pl. III, and Pl. IV, fig. 1.

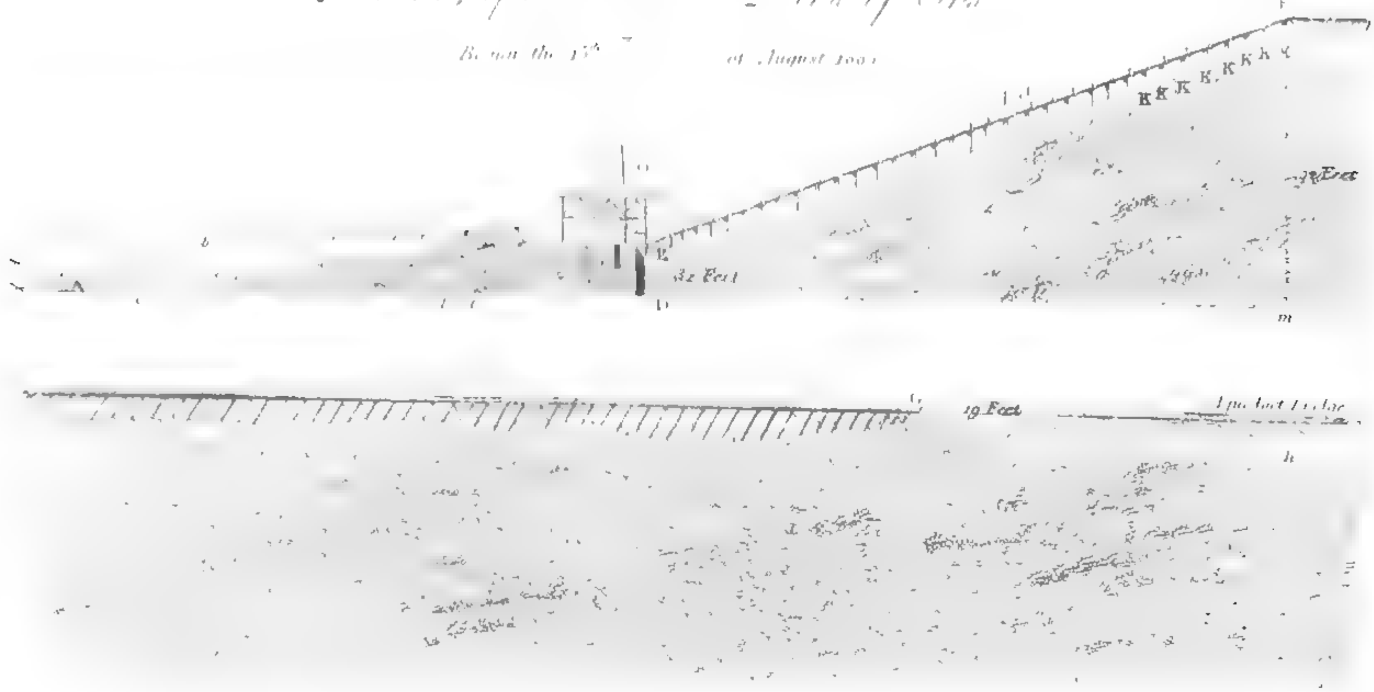
Explanation of the plates.

A. The pond of Citis.

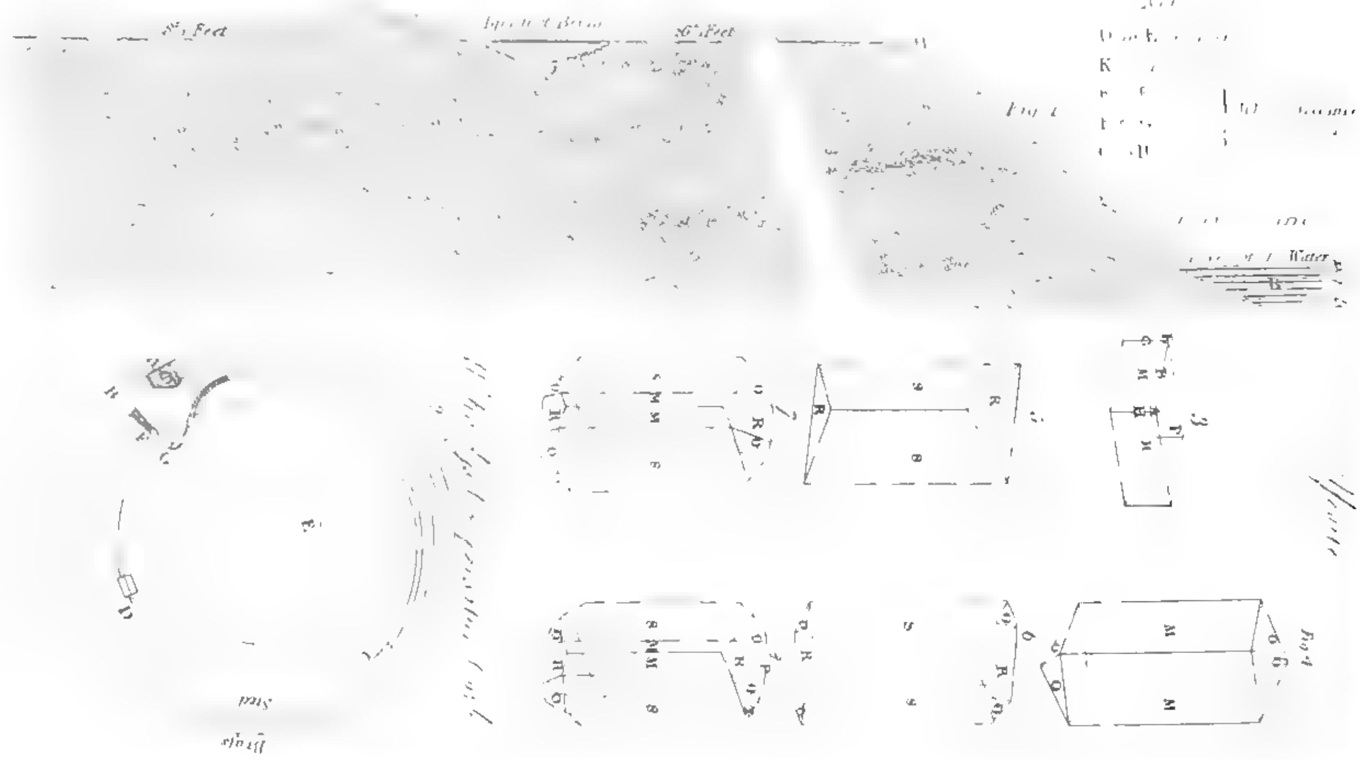
B. The arm of the sea, called the pond of Berre.

a. Level of the pond of Citis.

b. Level



Draining of the Pond of Cita



b. Level of the pond of Berre.

C D. Gallery that conveys the water from the pond of Citis into the well.

D E. The well, in which are the pumps.

E F. The cast iron cylinder.

F G. The wooden trough.

G H. The canal cut through the rock.

O. The steam engine.

g, *h*. Aqueduct bridges.

K K K K. Pillars of mason-work, supporting the iron cylinder.

K *m*. Height of the first hill.

III.

Remarks on some Pseudomorphoses observed in the Substances, that form Part of the Mineralogical Collection of the Council of Mines: by Mr. TONNELIER, *Keeper of the Mineralogical Cabinet to the Council**.

MINERALS that crystallize regularly do not always appear under those figures, that may be considered as appropriate to them. Frequently they assume those of organized bodies, and sometimes those of substances included like themselves in the mineral kingdom, but of a different nature. These borrowed forms have been designated under the names of *pseudomorphoses*, or *pseudocrystals*; and these are the more suitable, because, if they do not always deceive us, they may at least under certain circumstances impose upon us with respect to their real origin. In some cases too they present us with enigmas not easy to explain, since we cannot always conceive what substance it is, the natural figure of which they have borrowed, though we soon detect those that have assumed it, under the mask by which they are concealed.

Stones assume forms not their own.

These called pseudomorphoses, or pseudocrystals.

The pseudomorphoses I have chiefly in view in writing these observations are thus far remarkable, that they appear in minerals,

Steatite and serpentine appear in the

* Journal des Mines, No. 116, p. 155.

form of crystals.

nerals, that are commonly amorphous, as the steatite, *specksteín* of Werner, and serpentine. Nature, which very rarely indeed allows the species, of which these are varieties, to assume its proper forms, seems to have designed to indemnify it, by placing it in situations favourable for borrowing those of certain species, which appear to yield them up with more readiness, in proportion as they are more susceptible of variation.

Different crystalline forms assumed by steatite of Bayreuth.

The steatite of Bayreuth, of which there are many specimens in the collection of the Council of Mines, sent by Dr. Scheider, a physician at Hoff, in Franconia, exhibits several pseudomorphoses. The chief of these are the primitive rhomboid of carbonate of lime; those of the equiaxal and inverse varieties of the same species, as well as the dodecaedron, with scalene triangular pyramids opposed base to base, the *metastatique* of Haüy; the hexagonal prism, terminated at each end by hexaedral pyramids, of the prismatic hyaline quartz, sometimes alternate, at others bisalternate and flattened.

Steatite from Carlsbad.

A steatite from Carlsbad in Bohemia has exhibited to Mr. Haüy a remarkable pseudomorphosis, consisting in an oblique prism with a rhombic base, similar to the binary feldtspar. It forms part of a rock with base of feldtspar, which serves it for a gangue. This interesting specimen was sent by prof. Jurine of Geneva, who has a duplicate of it in his collection.

Serpentine.

Mr. de Champeaux, to whom we are indebted for a knowledge of the situations, in which uranium, oxide of titanium, emeralds, and graphic granite are found in the department of the Saône and Loire, one of those which that engineer of Mines has the charge of inspecting, has found in the valley of Viège, at Mont-Rose, a serpentine interesting for the novelty of the regular figures it exhibits. This substance, which is in the collection of the Council of Mines, is of a greenish colour, a little transparent on the edges, and nearly approaches the noble serpentine of Werner. Oligist iron, or specular iron ore, is disseminated in it. It exhibits, besides, 1st. the form of the prismatic hyaline quartz, their being no difference that can be appreciated between the inclinations of the faces, and values of the angles, when they are compared together: 2dly. the same form modified by facets occupying the places of the edges contiguous to the summits of the pyramids, which had

never

never yet been observed in the quartz itself. All these pseudomorphoses of steatite and serpentine are so many examples added to the well-known instances of quartz, which borrows sometimes from one species, sometimes from another, forms it is incapable of assuming when left to itself.

Supposed origin.

The idea that first occurs to the mind respecting the origin of such accidental forms has been to suppose, that the species, which has lent its form, has had an influence on the crystallization; and though mixed with a foreign substance, often predominant in point of quantity, nevertheless acts the principal part, and impels its companion to yield to the form it imposes on it. Thus it was at first supposed, that, in the rhomboidal figures, similar to those of calcareous spar, exhibited by the steatite of Bayreuth, and the same might be said of the other forms imitated from carbonate of lime, there existed originally a certain quantity of carbonate of lime, as in the crystallized sandstone of Fontainebleau, and that the steatite owed its form to this carbonate.

That a portion of the crystal imitated has been able to determine the form of the substance mixed with it.

Subsequently however the pseudocrystals of steatite have been compared with the steatitic mass, by which they are completely enveloped; and they have been found perfectly similar in every respect to the gangue in which they occur, possessing its softness, greasy appearance, soapy feel, &c. No trace of the substance, the presence of which was supposed necessary to imprint on it the regularity of form that distinguishes it, could be perceived. These considerations, and the difficulty of explaining how carbonate of lime, hyalin quartz, and feldtspar, could yield their place to the steatitic particles, allowing them to arrange themselves in the precise order required for the regularity of the figures retained, have appeared a motive sufficient to consider these forms as proper to the substances bearing them. Analogy, however, and the usual laws of crystallization, appearing to me little favourable to this opinion, I shall submit my doubts on the subject in a few words.

But the false crystal does not appear to contain any such portion.

Difficult to explain how steatite should take the place of a preexisting crystal:

hence supposed to be an original steatite crystal.

We frequently see quartz assume the cubic or octaedral form of fluate of lime, at others affect that of the metastatic carbonate of lime, and again put on several of those of sulphate

Spurious crystals of quartz.

phate of barytes*. The origin of these forms is by no means equivocal.

Pseudocrystals of quartz,

* In the collection of the Council of Mines are several quartzose pseudomorphoses, of which I shall content myself with mentioning the most remarkable. The first is borrowed from the metastatic carbonate of lime, and was found at Montbrizon, in the department of the Loire, by Mr. Laverrière, engineer in chief. The origin of this accidental form is by no means enigmatical. It is even necessary, in order to account for it, to have recourse to a sort of cementation, by which the particles of quartz would gradually have taken the places of those of the carbonate of lime, which before occupied the situation; it is sufficient, that a cavity left void by the calcareous spar, destroyed by any cause, served for a mould to the matter of the quartz. A piece of calamine, from Somersetshire, which is in the systematic collection of the Council of Mines, exhibit a pseudomorphosis similar to that of the quartz of Montbrizon. The pseudocrystals of this ore of zinc are of a reddish brown colour, three inches long, and hollow within, a circumstance in which they differ from the preceding, those being full and compact. The crystals of metastatic calcareous spar, which are sometimes found in the interior of those of calamine, and certain groupes of similar calcareous spar mentioned by Romé de l'Isle, part of which is still in the state of carbonate of lime, while the rest is in that of oxide of zinc, leave no doubt respecting the origin of this pseudomorphosis.

of calamine,

The department of the Saône and Loire, and that of the Nievre, visited by Mr. Champeaux, have afforded a variety of pseudomorphoses of a quartzose nature. These forms, all borrowed from acidiferous substances, derive their origin in some instances from fluate of lime, in others from sulphate of barytes. The regular forms borrowed from fluate of lime are the octaedron and the cube. These octaedrons are either hollowed out, or in relief. The faces of the first are plane, or convex: the second exhibit sometimes a regular octaedral summit, at others a simple equilateral triangle. The cubic forms, which are more numerous, are either solid or hollow. All these forms exist with the same appearances in the fluor spars found in the same place. The forms originating from the sulphate of barytes are the primitive form of that sulphate, with the trapezoid, the pointed, the laminar, the concrete, and the radiated varieties. The pseudomorphic quartz crystals originating from sulphate of barytes are not accompanied with this sulphate, as those indebted to fluate of lime for their form are with this fluate; whether because the sulphate of barytes has been subsequently destroyed, or because the pseudomorphic quartz has been removed from its place; which must have happened sometimes, since it is found not only in veins, but in ravines, and on the surface of the ground. However, on proceeding but a little way from the places where these pseudocrystals of quartz are found, we soon meet with veins of sulphate of barytes, and this in sufficient abundance, to leave no doubt of the origin of these pseudomorphoses.

of quartz.

If

equivocal. The fluate of lime, sulphate of barytes, and carbonate of lime, which are found in the same places, are so many faithful witnesses, which point out the source whence these forms are derived: and though we are not able to explain completely every circumstance respecting them, their nature cannot be doubted. When we find steatite exhibiting itself under several of the forms of carbonate of lime, may we not with great probability infer, that it has only imitated quartz by deriving from the same source the forms common to both? and when it presents itself under the forms that belong to quartz, is it not highly probable, that these forms are no more peculiar to it, than those of carbonate of lime are to quartz?

Steatite.

But it may be said, the crystals of steatite so perfectly resemble the mass in which they are enveloped, that we must suppose them to be the same substance, differing only in regularity of form. To this I would answer, such an inference is contradicted by analogy: for, when a substance is regularly crystallized, and its crystals are enveloped in an amorphous mass serving as their matrix, this is commonly of a different nature. Thus fine limpid crystals of hyaline quartz with two points are found buried in white Parian marble, in certain clays or marles, and in porphyries; crystals of hematoid quartz, or red jasper, and of borat of magnesia, are concealed in masses of gypsum; crystals of sulphate of lime are commonly found in banks of clay; crystals of specular iron ore, garnet, tourmalin, and magnesian limestone, occur in micaceous schist; &c.

Crystals do not form commonly in a mass of the same substance.

Instances.

It may be said farther, that the steatite, which exhibits forms analogous to those of rock crystal, presents others, that appear to be peculiar to itself; such for instance as the hexagonal prism with hexaedral pyramids truncated on the edges contiguous to the summit, which raises the number of terminal faces to twelve. This observation, I confess, might have been adduced as a very plausible objection, before quartz had shown

Steatite in a peculiar form:

but this has since been

If these pseudomorphoses of our departments be compared with these of Saxony, Bohemia, and Hungary, described by baron von Born, we shall find, that they present the same circumstances of form and situation, and have a similar origin.

found in quartz.

shown us in the crystals of the geodes of Oberstein this very secondary form, the structure of which, as ascertained by Mr. Haüy, is derived from the primitive rhomboid of quartz. But since this variety of form, which has not escaped the attentive eye of Mr. Tondi, occupies a place in the series of forms of quartz, the difficulty vanishes, analogy resumes all its weight, and the origin I ascribe to the regular forms of steatite retains it probability.

Argument from the laws of crystallization.

The laws of crystallization have been appealed to in favour of the opinion I combat. On breaking the steatite of Bayreuth, we discover in it parts, which have the form of the rhomboidal calcareous spar. It is in fact the primitive rhomboid of carbonate of lime, which has been mentioned above as one of the forms, under which steatite sometimes presents itself. Now it has been said, rhomboidal molecules are capable of producing the prismatic form of rock crystal, and that of the inverse calcareous spar, the muriatic calcareous spar of de l'Isle: therefore, the forms observed in steatite may be its own. It is very true, that the obtuse rhomboid of $101\frac{1}{2}^{\circ}$, similar to that of carbonate of lime, performing the office of a nucleus and substractive molecule, may produce the hexaedral prism of rock crystal. It does this in the prismatic carbonate of lime, by means of a decrement on the inferior angle of the nucleus in which two rows of molecules are subtracted; and this law is general for every rhomboid. But it cannot produce the hexagonal pyramid, which terminates the prismatic hyaline quartz, with the same incidences which are constantly found in the quartz; as these require for the primitive form and subtractive molecule a slightly obtuse rhomboid only, the angle of which is about 94°.

Proof against this.

Carbonate of lime crystallized in the same figure, but with different angles.

Mr. Héricart Thury, engineer of mines, has found near Grenoble indeed carbonate of lime crystallized in a hexaedral prism with a pyramidal summit of six triangular faces; but this form has nothing in common with the prismatic quartz, the crystals being altogether different, both in respect to the incidences of the faces, and the values of their angles. It differs from the prismatic hyaline quartz, as the greenish yellow phosphate of lime in hexaedral prisms terminated by hexagonal pyramids, the *spargelstein* of Werner, differs from the

the two former, and from the phosphate of lead, which sometimes assumes an analogous form.

In combating the opinion of those, who might be tempted to consider the regular figures under which the steatite of Bayreuth and the serpentine of Mont-Rose present themselves as crystalline forms properly belonging to these substances, I have not concealed the difficulties, to which the opposite opinion is obnoxious. I frankly confess the impossibility of conceiving, for want of local facts and observations, the means that nature can have employed for destroying the quartz crystals, which I suppose to have been originally included in the steatite, and fragments of which are found in neighbouring masses of steatite, to supply their place subsequently by a mass similar to the gangue in which they are included, yet so as to retain the ancient figure. I know not any rational explanation, to account for what has become of the substances, the forms of which alone remain. It appears to be a secret, which nature has preserved; but which farther observations, and inspection of the places, may perhaps some day enable us to penetrate. If however we believe the existence of nothing, except what we can completely explain, how narrow must be the bounds, to which we confine our knowledge!

Still it is difficult to conceive how the quartz crystals were destroyed, and the steatite assumed their place.

IV.

An Experiment on Soap-Suds as a Manure. By Mr. G. IRWIN, *of Taunton; with Remarks by the Rev.* THOMAS FALCONER*.

A Few years ago my attention was attracted by the soil of a garden, reduced to a state of poverty very unfriendly to vegetation. Interest in its future produce influenced my wishes for its restoration. An invigorating manure was necessary; but such a stimulus could not be easily procured. While considering which of the succedanea within my reach

Soil of a garden become poor

* From Papers of the Bath and West of England Society, vol. XI, p. 261.

had the greatest probable appearance of succeeding, it occurred, that possibly some trivial advantage might be derived from the oil and alkali suspended in the waters of a washing †. Pits were immediately ordered to be made, and in them the contents of a tub, which my servant usually committed to the common sewer, were carefully deposited: as washing succeeded washing, other pits were dug and filled; so that the whole garden, a small portion only excepted, has in this manner been watered and enriched: that small portion remains a visible demonstration of the utility of this manure. There vegetation is still languid; while the residue of the garden, invigorated by the suds only, annually exhibits a luxuriance almost equal to any thing this fertile neighbourhood can produce.

enriched by soap-suds.

I am, Sir, your humble servant,

GEORGE IRWIN.

Remarks, by the Rev. T. Falconer.

Dr. Hunter's oil compost.

1. The above important experiment may perhaps remind the reader of the principal ingredients of the oil compost, suggested by Dr. Hunter of York. In the simple fluid manure we have an animal oil, potash, and water; in the compost are the same oil and the same alkali, but neither of them perhaps in so pure a state as in the manure, with the addition of "fresh horse-dung." The fresh horse-dung is added, in order to produce "heat and fermentation;" and a delay of "six months" is supposed to be necessary, to make the compost "fit for use." All, however, that seems to be gained by the horse-dung, is the animal oil, which may be united to the alkali during the process of fermentation, and the straw, which in the fermentation of the compost will bind the mass together, and when decomposed on the ground will afford a small supply of vegetable matter. If we make the comparison strictly accurate on the

† It is the common practice of some parts at least of the west of England, to use a lixivium, made by passing water through an appropriate strainer containing wood ashes, for the purpose of washing. This was probably the case here, though not mentioned by the author.

other

other side, we may observe, that in the fluid manure there must be an increased quantity of animal matter in the water, after it has been used for the purpose of washing linen.

The experiment then shows what is the advantage of the application of the oil and alkali only, as a manure, and perhaps the delay of "six months" in preparing the compost would not be compensated by any superior efficacy, that may be expected to arise from the combination of the horse-dung.

It also appears from the experiment, that the compost is a more useful discovery than Dr. Hunter himself could justly infer from his own limited experience of its effects.

2. This mixture of an oil and an alkali has been more generally known than adopted, as a remedy against the insects which infest wall-fruit trees. It will dislodge and destroy the insects, which have already formed their nests and bred among the leaves. When used in the early part of the year, it seems to prevent the insects from settling upon them; but whether by rendering the surface of the leaf disagreeable to the bodies of the animals, and thus repelling them, or by neutralizing the acid they deposit, and thus preventing the leaf from contracting into a necessary form for their reception, I cannot presume to determine. One of the modes, by which this mixture indirectly contributes to the fertility of the ground, may be by its destruction of the insects, which prey upon the plants.

Soap-suds a remedy against the insects that injure fruit-trees.

It is also, I think, to be preferred to the lime water, or the wood ashes and lime, which Mr. Forsyth recommends to be used for the removal of insects. It is preferable to the lime water and the lime, because lime loses its causticity, and with that its efficacy, by exposure to air, and must consequently be frequently applied; and to the dredging the leaves with the fine dust of wood ashes and lime, because the same effect is produced by the mixture without the same labour, and is obtained without expense.

Preferable to lime water, or caustic lixivium.

Mr. Speechley, in his treatise on the Vine, published in 1796, has used this mixture with great success; but he has applied it awkwardly and wastefully. He directs it to be poured from a ladder out of "a watering pot over both trees and

Mr. Speechley recommends it.

and wall, beginning at the top of the wall, and bringing it on in courses from top to bottom:" page 161. Mr. Speechley is not the first person who has thought of this application of the mixture. It is a fact which has been long known and neglected.

Best applied by a garden engine.

A considerable extent of wall may be washed by means of a common garden pump in a short time; and this operation should be repeated as often as a supply of the mixture can be procured; or if the water of a washing cannot be had, a quantity of potash of commerce dissolved in water may be substituted*. The washing of the trees and wall twice a week for three or four weeks in the spring will be sufficient to secure them from the injuries of these insects.

A valuable manure for the farm as well as the garden.

On the whole, then, this must be considered as a valuable manure, as it can be obtained easily, at small expense, and in large quantities; and, when its nature is well understood, will probably be no less esteemed by the farmer than horse dung. To the gardener, as well as to the farmer, it is useful, mixed with mould, as a fertilizing compost; or, when fluid may be applied to his fruit-walls, as a wash fatal to the noxious brood of predatory insects.

THOMAS FALCONER.

V.

An Inquiry into the Causes of the Decay of Wood, and the Means of preventing it. By C. H. Parry, *M. D.*

(Concluded from p. 78.)

Would the varnish in some cases admit the growth of fungi?

I Do not know whether in very damp situations, surrounded with stagnant air, these varnishes would in time admit of the growth of fungi or mould. The brimstone might be sufficient to preclude that effect; but, if we believe Braconnot, seeds of the white mustard sown in pure flowers of brimstone, and well watered, became vigorous plants, which

* Mr. Speechley uses his mixture warm, to soak the shreds, and wash the wall, more effectually.

flowered

flowered and produced effective seed *. It is certain, however, that the essential oil of turpentine will act as a poison on growing vegetables; and perhaps the same property may exist in resin, which seems to be a similar essential oil, united with a certain proportion of oxigen.

It is however highly probable, that the union of the brimstone may have another good effect, which is to prevent one of the causes of the destruction of timber which I have before mentioned, the depredations of insects. Whoever would learn the havoc, which certain animals of this kind are capable of making in hot countries, would do well to read Smeathman's description of the termes, or white ant, originally published in the Philosophical Transactions, and thence abridged into the English Encyclopedia Britannica, and other collections. In this country we know little of such ravages. Mischief however of this kind does sometimes occur, and may be the work of various animals, a particular account of which may be met with in the fifth volume of the Transactions of the Linnæan Society. The brimstone may defend from insects.

I am informed, that in India, a circle of Lord Dundonald's coal tar drawn on the floor round boxes and other furniture, will effectually preserve them and their contents from the depredations of the white ant. Coal tar.

It appears, that most insects are fond of sugar and mucilage; which is the probable reason why that wood is most subject to be penetrated by worms, which is felled when it most abounds with sap. In such cases, it might be well to try the effects of washing the wood, previously to the use of the varnish, with a solution of arsenic in hot water, in the proportion of 1 lb. to 10 gallons; or with a strong decoction of coloquintida or bitter apple, or white hellebore; after which the wood must be completely dried before the application of the varnish in the manner before directed. All these preparations are extremely cheap, and are either destructive or offensive to insects, and therefore will, probably, be an effectual defence against any injury from that cause. Other defences against insects.

C. H. PARRY.

Circus, Sept. 30, 1807.

* See Journal, vol. XVIII, p. 18.

VI.

VI.

Analysis of Jade; read to the Society of Natural History and Philosophy at Geneva, Dec. 5, 1805: by THEODORE DE SAUSSURE *.

General characters of jade.

UNDER the name of jade are generally comprised certain stones, not crystallized, remarkable for a greasy or oily appearance; a colour between waxy white and leek green, inclining sometimes to a blue, sometimes to a gray; a dull, greasy, scaly, and not lamellar fracture; extreme tenacity; hardness capable of scratching rock crystal; and lastly, a density superior to that of feldtspar or petrosilex.

Two stones possess these: the oriental, or lapis nephriticus;

Two stones, which have been considered only as varieties of the same species, unite all these characters in an eminent degree. One of these is the oriental jade, or *lapis nephriticus,* which Mr. Haüy calls *jade néphrétique.* This comes from China and the Levant, but we know not its situation in the earth. It is celebrated for the property ascribed to it by the Eastern nations of curing the renal colic, and allaying the pain of the stone. It is known in Europe only by the amulets, vases, and other pieces of sculpture brought from the places where it is native.

and one found in Europe, tenacious jade.

The other, considered by most mineralogists as a variety of the oriental jade, is found in several parts of Europe. My father was the first who made it known, after having found it on the borders of the Leman lake *(Voyages dans les Alpes,* § 112*)*, on those of the Durance, at Musinet near Turin, and in other places. From the name of the lake it was called *lemanite* by Mr. de la Métherie, who has well distinguished it from the oriental jade. Mr. Haüy has called it *tenacious jade;* and several authors have mentioned it by the name of Saussure's jade. This stone resembles the oriental jade in colour, hardness, tenacity, and fracture: but it differs in its specific gravity, which is greater; in its transparency, which is less; and in its fusion, which is more easy, and affords a perfect glass, with a smooth, conchoidal

Characters of this stone.

Differs from the oriental.

* Journal des Mines, No. 111, p. 205.

fracture,

fracture, though frequently semitransparent, while the oriental jade produces only an opake mass, with a dull, uneven, and by no means conchoidal fracture. It differs likewise, as I shall show presently, in its constituent principles. It is proper therefore, that the name of jade should be taken from it; and I would propose to substitute that of Saussurite, as a compliment to the memory of my father, who first directed the attention of mineralogists to this stone. Names too, like this, which have no particular signification, are most convenient, because they do not lead us into errour. Names derived from one of the places where a stone is found are always improper, as has frequently been remarked, because it is not peculiar to this place exclusively. Names derived from one of the characters of a fossil too, in whatever language they are framed, are not more suitable; since this character never belongs exclusively to the mineral denoted by it, which differs from others only by its general properties.

Name of Saussurite proposed for it.

Names should not have a determinate meaning.

Werner considers as a subspecies of jade the *beilstein*, *pierre de hache*, or axestone, which is chiefly known to us by means of the hatchets fabricated with it by the Americans. But this is much inferior in hardness and density to the stones generally comprised under the name of jade, and does not easily strike fire with steel; though it has a greasy appearance and greenish colour. On this stone however I can say nothing more, as I have it not in my possession, and have been able to examine it only superficially, so that I am obliged to leave its rank undetermined.

Beilstein considered as a jade.

The greasy polish of jades has appeared to most mineralogists to indicate, that they are impregnated with talcy particles, and that consequently they ought to be classed with the steatites. Mr. Hoepfner has confirmed this opinion by the analysis he has given of the jade of Swisserland. In this he found 0.47 silex, 0.38 magnesia, 0.04 alumine, 0.02 lime, and 0.09 oxide of iron. The magnesian nature of this stone appears the better founded, as it sometimes occurs in mountains of serpentine: but I thought it necessary, to repeat the examination, partly because this was made at a time when processes were less precise than at present; partly because

From their greasy appearance supposed magnesian: and Hoepfner's analysis gives 0.38 of magnesia in the Swiss jade; but this questionable.

because the identity of the tenacious jade and the oriental jade did not appear to me to be proved.

Analysis of the oriental jade, jade néphrétique of Haüy.

Analysis of the oriental jade. The specimens described.

For this analysis I employed amulets cut in form of a crescent very little hollowed out. Their colour was a leek green, inclining to gray: their specific gravity 2·957. According to Brisson the specific gravity of this jade is 2·966*; and according to my father between 2·970 and 3·071.

These amulets are interiorly dull, and merely shining in small spots; they exhibit a dull fracture, with some fibres here and there, either straight or curved; they are semitransparent, and hard enough to scratch rock crystal, but are scratched by the topaz and the emerald. Their tenacity is very great: I could not pulverise them without greatly injuring an agate mortar, till I heated them red hot, and threw them into water. In a red heat they lose all their transparency and about $\frac{1}{200}$ of their weight, their green colour changes to a dark dirty gray, and they become fragile.

Exposed to a strong heat in a platina crucible.

1. One of these amulets, of the weight of about 6 grammes [93 grains], was exposed whole for an hour in a platina crucible to the most violent fire of a wind furnace. It there melted into a button, which was gray on the surface exposed to the air, but white interiorly; opake, being merely a little translucid at the edges; of a greasy, unequal, and confusedly lamellar fracture; and covered here and there with smooth, shining, greasy crystals, the extremity of which only was visible. This extremity exhibited very flat pyramids with four faces, the two larger of which terminated at the summit of the pyramid in two obtuse angles, and the two intermediate in acute angles. The upper surface of the button, when inspected with a microscope, showed a multitude of metallic globules of a gold colour, the nature of which I could not ascertain. The lower surface was covered with a row of large blebs, that did not penetrate into the substance. A small part of this button was fused before the blowpipe, but without forming a glass. One hundred parts of the jade by weight lost by fusion $2\frac{1}{4}$ parts.

* In Brisson's Mineralogy it is from 2·9502 to 2·9829. Tr.

2. I

2. I exposed to a red heat for two hours a mixture of 100 parts of this jade pulverised with 450 parts of potash. The result was a deep grass green mass, not vitrified, that communicated the same colour to cold water, in which it was diffused. This colour soon disappeared, the solution at the same time letting fall a gray flocculent precipitate, which afterward became brown. These defects indicated the presence of oxide of manganese, which for the present I left mixed with the other principles of the stone.

Heated with potash, and water affused.

3. The preceding liquor, as well as the undissolved part, was mixed with a portion of muriatic acid in excess; but this did not attack a brown or blackish flocculent residuum, which, being mixed with thrice its weight of potash, produced on exposure to the fire a green glass. This dissolved entirely in water and muriatic acid. The muriatic solutions being mixed and evaporated yielded a jelly, which being reduced to dryness, and the residuum digested in muriatic acid diluted with water, $53\frac{3}{4}$ parts of pure silex, distinctly characterized, were obtained.

Muriatic acid added.

4. The muriatic solution, separated from the silex, was mixed with ammonia; and a yellow precipitate formed, consisting of the metallic oxides and alumine. This precipitate, while still wet, was digested with potash twice in succession, to dissolve the alumine: but this solution, when supersaturated with acid and precipitated by ammonia, threw down but half a part of alumine.

The muriatic solution precipitated by ammonia.

5. The metallic oxides left on the filter after such a process as the preceding are seldom pure, as they retain both alumine and alkali. To separate these, they were mixed with five times their weight of potash, and heated red hot. The result was quickly diluted with cold water, and thrown on a filter, which retained the oxide of iron; a green liquor, holding in solution alumine and oxide of manganese, passing through. The oxide of manganese, precipitated by boiling the solution, weighed when dry half a part. The solution, after this oxide was separated from it, being supersaturated with acid, and precipitated by ammonia, some alumine was thrown down, which when dried at a red heat weighed one part.

Metallic oxides heated with potash, & cold water affused.

Manganese precipitated by boiling:

alumine by ammonia.

The oxide of iron, being freed from the alkali, that remained

Oxide of iron precipitated.

mained united with it, by dissolving it in muriatic acid, was precipitated by ammonia. After calcination it weighed six parts and half. But as its black colour indicated, that it still retained some oxide of manganese, I digested it repeatedly with vinegar, evaporating it to dryness every time, and redissolving the residuum in water. The solutions being added together, and precipitated by potash, yielded 1½ part of oxide of manganese: the pure oxide of iron therefore weighed but 5 parts.

Some more oxide of manganese separated from it.

6. The muriatic solution (3) separated from the alumine and metallic oxides was supersaturated cold with carbonate of ammonia. This separated 22 parts of carbonate of lime, which furnished after calcination 12¾ parts of pure lime. The ammoniacal liquor, being filtered, let fall nothing on ebullition.

Carbonate of lime precipitated by carbonate of ammonia.

The 12¾ parts of lime I dissolved in sulphuric acid, and digested in water: they were found to have the same degree of solubility as sulphate of lime, and I could not discover, either by crystallization, taste, or any other sign, an atom of sulphate of magnesia.

No magnesia could be discovered among it.

Thus a hundred parts of nephritic jade yielded me on this occasion

Products.

Silex	53·75
Lime	12·75
Alumine	1·5
Oxide of iron	5
Oxide of manganese	2
Water	2·25
	77·25
Loss	22·75
	100.

This loss being much too great to be ascribed to an error in the process, I repeated the analysis in the same manner, endeavouring in addition to detect the presence of any of the acids, that sometimes enter into the composition of minerals.

From the great loss an acid suspected,

After this examination, which was so far fruitless, though in other respects it confirmed the preceding, giving nearly the

but none found.

the same results, though from different specimens, I sought to discover an alkali in the amulets, by employing nitrate of barytes to decompose them according to Klaproth's method.

Examined for an alkali.

A hundred parts of nephritic jade were mixed with five times their weight of nitrate of barytes. This mixture I divided into four parts; and after having exposed the first to the action of the fire in a platina crucible till it ceased to swell up, I added to it the second, and so on with the rest. The whole, after having been exposed to a red heat for at least half an hour, exhibited a spongy mass of the colour of goose-dung. This was pulverized, and diluted with a large quantity of cold water. The mixture assumed a lilac red colour, which disappeared by a boiling heat, but returned on adding a few drops of muriatic acid, and again disappeared on adding a farther quantity of the acid, which gave the liquor a yellow colour. It contained a white insoluble powder, weighing 43 parts. This powder was exposed to the fire with four times its weight of barytes; and the spongy white substance thus produced dissolved completely in water and in muriatic acid, without exhibiting the colours mentioned above.

Heated with nitrate of barytes.

Water affused, and muriatic acid added,

The muriatic solutions having been mixed together, sulphuric acid was added in excess, which separated the barytes, and part of the silex.

The solution precipitated by sulphuric acid.

The liquor was filtered, and evaporated, till all the muriatic acid was distilled off. The residuum moderately dry was digested in distilled water, which dissolved the whole, except the last portions of silex, and a little sulphate of lime.

The muriatic acid driven off by heat.

Residuum dissolved in water

The solution being filtered, ammonia was added, which precipitated the alumine and metallic oxides.

and precipitated by ammonia.

These substances having been separated, the liquor remaining after filtration was evaporated, and the residuum heated to redness. This, which was of a whitish colour, weighed 56 parts. Being diluted with cold water, 16 parts of calcined sulphate of lime were separated by filtration. The alkaline sulphate therefore weighed after calcination 40 parts.

The liquor evaporated,

and earthy sulphates separated.

The

The alkaline sulphates separated by crystallization.

The aqueous solution of alkaline sulphate, being left to crystallize slowly, showed itself to consist of sulphate of soda and sulphate of potash. These salts when crystallized weighed 74 parts. The sulphate of soda after calcination weighed 24·6 parts; that of potash 15·4 parts. Assuming for these salts the proportions assigned by Kirwan, we find, that the stone contained 10·83 parts of soda, and 8·44 parts of potash.

Component parts of oriental or nephritic jade.

On putting together these results, we find, that 100 parts of nephritic jade contain

Silex	53·75
Lime	12·75
Alumine	1·5
Oxide of iron	5
Oxide of manganese	2
Soda	10·75
Potash	8·5
Water	2·25
	96·5
Loss	3·5
	100.

Differs from all other stones.

Hence the nephritic jade appears to have no resemblance to any stone hitherto analysed.

Analysis of the Saussurite, tenacious jade of Haüy.

Specimen of saussurite, or tenacious jade, described.

For this analysis I selected a rounded pebble, found on the borders of the lake of Geneva by my father, who considered it as a pure and well marked jade. Its colour was a deep leek green inclining to sea green. Its surface, polished on one side by art, on the other by natural attrition, was smooth, shining, oily to the sight, and greasy to the feel. Its fracture was dull, not lamellar, fine-grained, and with large scales. On the edges it was translucid. Its tenacity was very great, and similar to that of the nephritic jade. It easily scratched rock crystal, but was scratched by the topaz and the emerald. Its specific gravity was 3·261. That of specimens weighed by my father was 3·318, 3·327, and 3·389. It was free from diallage, or smaragdite, which is almost always

ways found disseminated in it. It had no perceptible effect on the magnetic needle.

A saussurite very distinctly marked yielded before the blowpipe a greasy, semitransparent glass, of a white or greenish colour: but the same stone, which in this way produced such a glass, being exposed to the most violent heat of a wind-furnace in a platina crucible for an hour, yielded a light brown glass, of the most perfect transparency, and free from blebs both within and at the upper surface. Some were seen in contact with the sides of the crucible. I thus fused about six grammes of saussurite, which did not lose by this operation any sensible portion of its weight *.

Fused into a pale semitransparent glass; & at a greater heat into a perfectly transparent brown glass.

I shall not detail the processes I employed to analyse this stone, since they were the same as those already described. I shall only mention, that, to separate the alkali, I attempted to treat the powdered saussurite with sulphuric acid, by boiling it on it, and evaporating to dryness. I repeated this process with the residuum six times, powdering it each time. But I could not by this process extract above 0·12 the weight of the stone, or deprive it of more than 0·02 of alkali. I then treated with nitrate of barytes, assisted by heat, the insoluble part, which had retained the metallic parts, because it had been calcined. The spongy matter procured by this operation was of a greenish gray. Cold water did not bring out the lilac colour, which had appeared on treating the oriental jade in the same manner. This colour was owing probably to the oxide of manganese, which exists in some quantity in the oriental jade, but was scarcely sufficient to be weighed in the specimen of saussurite, that I analysed.

Analysed as the other.

Attempt to separate the alkali by sulphuric acid unsuccessful.

* On this glass free from blebs I made one striking observation. It was, that the specific gravity of the stone previous to fusion is much greater than that of its glass. The specific gravity of the saussurite is 3·261: that of its glass is at most 2·8. The glass is softer than the stone, and easily scratched by it.

The glass lighter & softer than the stone itself.

A hundred

Component parts of the saussurite.

A hundred parts of saussurite afforded me

Silex	44
Alumine	30
Lime	4
Oxide of iron	12·5
Oxide of manganese	0·05
Soda	6
Potash	0·25
	96·8
Loss	3·2
	100.

It is neither a magnesian stone, nor a jade.

From these results it appears, that the saussurite is not a magnesian stone. It appears too, that it cannot be classed with the nephritic jade, as the alumine, which is in very small quantity in the jade, forms a considerable proportion of the saussurite; and the two stones likewise differ greatly in the alkali they contain.

The saussurite compared with feldtspar.

The saussurite contains a great deal more metallic oxide than feldtspar; their earthy principles however are the same: at least they succeed each other in the same order, the proportion of silex only being greater in the feldtspar, and the proportion of alumine less. Their external characters, if we consider the extremes of the two species, are totally different, but there are gradations between these, that bring them almost together. Thus that feldtspar, which my father called greasy *(Voyages dans les Alpes*, § 1304*)*, and which is found crystallized in the green antique porphyry called *ophites*, and confusedly crystallized in nodules of variolite, does not always exhibit any signs of a lamellar structure. Its hardness is so great, that it readily scratches rock crystal; and like the saussurite it has a greenish and oily aspect.

If the granulous and scaly petrosilices be feldtspars, as analysis tends to show *, another link is added to connect them.

* See the analysis and description of the petrosilex of Pisse-Vache. *Voyages dans les Alpes*, § 1057.

I do

I do not intend by these gradations to confound the two stones: their elements, and their external characters, considered in the extremes, are sufficiently marked, to constitute distinct species. I would only remark, that they have shades of resemblance, which tend to confirm the results of analysis.

VII.

Remarkable Fact of an Increase of Temperature produced in Water by Agitation. In a Letter from JOSEPH READE, *M. D.*

To Mr. NICHOLSON.

SIR, *Cork, May* 8, 1808.

SINCE my communication on the increased capacity of water, I have been engaged with some experiments on *heat* excited by friction, one of which I beg leave to communicate through the medium of your Philosophical Journal, and hope it may not be esteemed uninteresting. I shall confine myself to a concise recital of the experiment, which if confirmed, is in direct contradiction to received opinion, that the agitation or friction of fluids cannot excite sensible heat.

Experiment.

Water at 40° rose to 48° by agitation in a closed tin vessel.

The temperature of the apartment being 40°, half a pint of water, at a similar heat, was poured into a tin bottle-shaped vessel; into the aperture of which was inserted a thermometer, surrounded with chamois leather, and made to fit accurately, with its bulb nearly in the axis. After briskly agitating the vessel for a few minutes, to my extreme surprise I found the temperature of the water rose 8 degrees; and even after the apparatus was uncovered and laid at rest on the table, the water continued to rise for several minutes; proving the origin of the heat to be inherent in the fluid, and independent of any external causes. Anxious however to obviate every source of fallacy or objection,

 I prevented

Repetition of the exp.

I prevented the communication of caloric by my hands, or of radiation from my body, by coating the tin vessel with many layers of woollen cloth carefully wrapped round it; over which there was a tin case, the entire nearly two inches in thickness, and covered externally with three wet towels. In the course of the experiment I dipped my hands frequently in snow water, and also sprinkled the towels.

Having repeated this experiment with similar results before the Rev. Mr. Hincks, Lecturer on Chemistry in the Cork Institution, I now venture to lay it before the public. Mr. Hincks on repeating the experiment in a glass bottle, found the heat of the vessel, by means of a thermometer placed between it and the covering, to be inferior to that of the enclosed fluid, and on a par with the atmosphere, which proves in a most satisfactory manner, that there could be no communication of caloric from the hands. Some extremely interesting conclusions may be drawn from this experiment. What is the cause of the increased heat? certainly not arising from a diminution of capacity. Is caloric material or immaterial? Is friction adequate to account for animal heat? Should this experiment on critical examination be found correct, these, and some other speculation on heat, will occupy a more extensive inquiry.

Sir, I have the honour to remain,

Your very obedient humble servant,

JOSEPH READE, M. D.

VIII.

Further Remarks on Professor VINCE's *Answer. By a Correspondent.*

Remarks on Prof Vince's letter upon gravitation.

IT is not the "mathematical," but the literary "abilities" of this country, that will be impeached, according to Professor Vince's ideas, by the observations contained in his answer; since "the errours in the works of Dytiscus" consist, if his explanation is admitted, in having first mistaken a plural number for a singular; and secondly, in having wantonly

wantonly understood a term in its common and only correct acceptation. But it appears to me, that the passage, which is the first subject of his critical remarks, admits, beside the two alternatives which he discusses, a third sense, essentially different from them both: "the two first terms of the series" may possibly allude to the two first terms of the *only* two series which are to be found in the essay, these two terms having already been mentioned as *sufficient* for determining the force: and if the author will take the trouble of reperusing the whole of his essay, instead of trusting to his memory for its general tendency, he will probably be aware, that such *must* have been his original meaning. Two of the four terms thus obtained destroy each other immediately after their birth; the other pair conspire in the production of a joint issue (p. 18); and this their offspring is precisely that which is honoured with a place in the 18th section, as the representative not only of both its parents, but also of the whole of the unfortunate family; for we are expressly and very truly told (p. 19), that the terms omitted are so small, that they could make no sensible alteration in the result. Let them rest in peace. Let not the same hand which has bestowed on them a decent interment as dead in Philosophy, now drag forth their poor remains to stand in dumb parade under the banners of Logic.

Remarks upon Prof. Vince's letter upon gravitation.

The series which Professor Vince now introduces to our acquaintance, as willing to present us with its two first members, is *not even mentioned* in the essay, much less so stated as to make it possible to found any reasoning upon it. If "it was proposed" to take any "second terms" of such a series into consideration, the proposal was wisely confined to the author's breast: for why should they be considered, if they could "make no sensible alteration" in the result? The series $\frac{\alpha}{a^2} + \frac{6}{a^4} + \ldots$ may certainly vary as $\frac{1}{a^2}$, if all the Greek letters after the first become inconsiderable, and our author has virtually confessed in his essay, that they do become inconsiderable.

His explanation not admissible.

As to the difficulty of extending the law to the internal parts of the sun's substance, it is perfectly obvious, that the law

Difficulty removed.

law of the density, as well as that of the force, must be supposed to change at the surface of every material body, long before $\frac{Q}{a}$ can become equal to P.

Atoms to be considered as separate.

Professor Vince's "two independent circumstances" are both dependent on the supposition of the external action of the medium on a material aggregate of considerable magnitude, which it never could have been in the contemplation of Newton to advance: it would be idle to maintain the possibility of the hypothesis on any other ground, than that of the independent action of the medium on every atom of matter. Here therefore he is fighting with a shadow, and not with "the vaunting assertions and errours of Mr. D."

Improper use of the term density.

It is difficult to perceive the "necessity" of employing the term density, in order to convey the idea of the square of the cube root of the density, simply because this was the *power* of the density that was required for the author's purpose. The density of light or heat diverging from a centre in the form of projected corpuscles, may be very justly estimated by the number of particles falling on a given surface, for this simple reason, that their number is here a true measure of the density; while in the case of an elastic medium it is not a true measure.

Atmospheres.

The idea of the interference of different atmospheres must be considered as in some measure foreign to the question, since only one general ethereal medium of variable density is supposed to be concerned, and since the modifications of this medium, produced by the several celestial bodies, might easily coexist without any material interference or interruption.

Mistake of another author.

I must beg leave to observe, on the other hand, that another modern author appears to me to have been somewhat too hasty in asserting, that the law of gravitation may be derived from the supposition of an elastic medium, repelled by a *force* which varies inversely as the distance. If I am not mistaken, such a force would produce, according to the common laws of the operation of forces, a medium varying in density as some given power of the distance, and an apparent attraction increasing with the distance of the material bodies concerned.

I have

I have been informed, that the only intimation commonly given to the author of a paper which is not to be printed in the Philosophical Transactions is a simple letter of thanks, without any further notice respecting it. But the Society does not usually return thanks for a lecture read by appointment: hence therefore must have arisen the omission, which Professor Vince seems to think so inexcusable.

Custom of the Royal Society.

I am sorry that any of your correspondents should have considered my remarks as written in an improper spirit: you, I believe, were not of that opinion; and I can only say, that if that correspondent could have pointed out to me any objectionable expressions, I should most willingly have omitted them. My only motive was the wish to repel an unjust attack; my observations tended more to impute inattention than inability to the party concerned; and I am at this moment ready to allow that a very great mathematician may not only be materially mistaken, but may resolutely defend his errour, when it is discovered by another person; and that he may even have so short a memory, as to forget, while he is defending himself, what he had before written on the same subject.

Intentions of the author.

I am, Sir,

Your very obedient servant,

7 *May*, 1808. DYTISCUS.

IX.

Calculation of the Rate of Expansion of a supposed Lunar Atmosphere. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IT has been a subject of inquiry among some who are attached to astronomical speculations, whether or no, if the moon had ever been possessed of an atmosphere equally dense with that of the Earth, she could have retained it, without a very sensible diminution, in consequence of the Earth's attraction, upon the supposition of the infinite dilatability of the

Inquiry into the effect of the Earth's attraction upon the atmosphere of the moon.

Inquiry into the effect of the Earth's attraction upon the atmosphere of the moon.

the air, with a density always proportional to the pressure. The inquiry involves a great variety of considerations, and it would be extremely difficult to make an exact calculation of all the particulars connected with it; but it may be shown from some general principles, that the diminution would have become perceptible to a spectator situated on the Earth, in the course of a few centuries.

Equilibrium of the joint atmosphere.

If a be the distance of the moon from the earth, and x the distance of any other point in the line joining them, the force of gravitation will be as $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2}$; and the centrifugal force, arising from the revolution round the common centre of gravity, to be added for the terrestrial atmosphere, and to be subtracted for the lunar, being equal to the force of gravitation at the distance of the centres, the joint force f acting on the particles of the atmosphere will be as $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2} + \frac{1}{70\,a^2}$, and $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2} - \frac{1}{a^2}$ respectively: or, since f must be equal to unity at the Earth's surface, when x is equal to the Earth's semidiameter b, $f = \frac{b^2}{x^2}$ near the Earth, without sensible errour, and $f = \frac{b^2}{x^2} - \frac{b^2}{70\,(a-x)^2} - \frac{b^2}{a^2}$, for the lunar atmosphere. Then the density being y, which may also be called unity at the Earth's surface, we have $-c\,\dot{y} = f\,y\,\dot{x}$, and it is obvious that c must express the height of a column of air of uniform density capable of producing the pressure by its weight, in order that $-c\,\dot{y}$ may be initially equal to $\dot{x}$. Hence we have H.L.$\frac{1}{y} = \frac{1}{c}.\int f\dot{x}$; but $\int f\dot{x} = b^2 \left(\frac{\dot{x}}{x^2} - \frac{\dot{x}}{70\,(a-x^2} - \frac{\dot{x}}{a^2}\right)$; therefore H.L.$\frac{1}{y} = \frac{b}{c}\left(d - \frac{b}{x} - \frac{b}{70\,(a-x)} - \frac{bx}{a^2}\right)$, d being, without sensible errour, $1 + \frac{b^2}{a^2}$. Now b is 3958, and c 5.28 miles, and at the moon's surface x is about $60\,b$, and $a-x$ $\frac{3}{11}\,b$; whence H.L.$\frac{1}{y} = 685.69$. Again, when f vanishes,

nishes, and the density is least, $\frac{1}{x^2} = \frac{1}{70\,(a-x)^2} + \frac{1}{a^2}$, and x is nearly $.825\,a$, whence $\mathrm{H.L.}\frac{1}{y} = 724.31$; and this density is to the density at the moon's surface as 1 to the number of which the hyperbolic logarithm is 38.62, and the common logarithm 16.773: and supposing the density to be increased in any given ratio, the proportion will remain the same, the number c still indicating the height of a column equal in density to the atmosphere, thus condensed, at the Earth's surface.

General law of motion.

Now the expansion of the lunar atmosphere, supposing it to be equal in density to that of the Earth, and to extend to the point where the force f vanishes, which is the most favourable condition for its permanence, may be determined from this general principle; that the motion of the centre of gravity of any system of bodies, some of which are urged by a greater force in one direction than in another, must be the same as if the difference of the forces acted on the whole system, collected into the centre of gravity. Thus, if the pressure of the highly rarified air, at the termination of the supposed lunar atmosphere, which would have kept it in equilibrium, be removed, the elasticity of the column pressing on the moon will be by so much greater than its gravitation; and the centre of gravity of the column will be repelled, with a velocity as much smaller than that of a body falling at the Earth's surface, as the pressure removed is smaller than the weight of the column: but this ratio is compounded of that of the densities at the opposite ends of the column, and that of the force of gravitation, or rather the force f, near the moon's surface, to its force at the surface of the Earth, since the mass required to produce the given density, by its pressure, is as much greater, as the gravitation is smaller; and if we diminish in this proportion the space which a falling body would describe in a century, we shall have 514 feet, for the elevation of the centre of gravity of a column of the lunar atmosphere in that time.

Centre of gravity of an elastic column.

But in order to estimate the effect of such a change, we must calculate the actual height of the centre of gravity of a given column of an elastic fluid: and for this purpose we

may

may suppose the attractive force uniform. The height of the centre of gravity is determined by dividing the fluent of $xy\dot{x}$ by the mass, or by $1-y$; but, since $-c\dot{y}=y\dot{x}$, $xy\dot{x}=-cx\dot{y}$, x being $=c$ (H.L.$\frac{1}{y}$), or, according to a mode of expression lately employed by one of your correspondents, $cm\,(\frac{1}{y}^{\frac{1}{m}}-1)$, when m is infinite; hence $-cx\dot{y}$ $=ccm\,(\dot{y}-y^{-\frac{1}{m}}\dot{y})$, of which the fluent is $e+ccm$ $(y-\frac{1}{1-\frac{1}{m}}y^{1-\frac{1}{m}})=e-cxy-cy^{1-\frac{1}{m}}$, or $e-cxy$ $-cy$; which must vanish when $y=1$ and $x=0$; consequently $e=c$, and the height of the centre of gravity is $c-\frac{cxy}{1-y}$; and when $y=0$, this height is equal to that of the column c, which for the Earth's atmosphere is 5·28 miles. and for the moon's as much greater as the force is smaller, that is, 27·75 miles. The centre of gravity being therefore elevated 514 feet, or $\frac{1}{280}$ of its height, in a century, the mean density of the column must also be reduced about $\frac{1}{280}$; but since a certain part of this elevation depends on the supposed acceleration of the more distant portions, which would produce no sensible effect in the neighbourhood of the moon, we cannot estimate the mean rarefaction of the part remaining more nearly in its original situation, at more than about $\frac{1}{300}$; and this will be reduced to about one fourth for the mean of the whole atmosphere, surrounding the moon on all sides: so that we may take $\frac{1}{1200}$ for the mean rarefaction of such a lunar atmosphere in the course of the first century.

Sensible effects.

So small a rarefaction as this would certainly not be directly observable at the distance of the Earth. Supposing that the atmosphere would be visible until its density became equal to a given quantity, the point, at which this density would be found, would be depressed only about 18 miles, if the whole density of the atmosphere were reduced to one half, and by a diminution of $\frac{1}{1200}$, only $\frac{1}{1200}$ of 27.75 miles, or about 120 feet. The effect of an atmosphere would however

ever be more perceptible in the refraction, which would occasion an alteration in the apparent place of a star about to be eclipsed, and which would amount, in the case of the Earth's atmosphere, to 66 minutes. But the refractive density of the lunar atmosphere would vary nearly as the 134th root of the distance, instead of the 7th; and the deviation, instead of 66 minutes, would become 13′ 50″, one 1200th of which would be only $\frac{7}{10}$ of a second, which would still be imperceptible; although in two or three centuries, since the rarefaction would increase at first as the square of the time, it might perhaps be discoverable; and this would be considerably sooner than the decrease of the moon's apparent diameter could be observed. It is however scarcely probable, that so slow a rate of diminution could have reduced the lunar atmosphere from a density equal to that of the terrestrial atmosphere, to its present state, in the course of 10,000 years. Lunar refraction.

I am, Sir,

Your very obedient servant,

16 *May*, 1808. HEMEROBIUS.

X.

Experiments on Molybdæna: by CHRISTIAN FREDERIC BUCHOLZ. *Translated from the German**.

IT is near thirty years ago, that the immortal Scheele discovered in molybdena, as it was then called, a peculiar metallic substance, many of the properties of which he made known, as well as its action on several other substances. Metal in molybdena discovered by Scheele.

Several able chemists, as Pelletier, Heyer, Ilsemann, Richter, Hielm, Klaproth, Ruprecht, and others, have since turned their attention to the same subject: but the knowledge we have acquired from their labours is by no means proportional to the number of chemists, who have examined it, and the time that has elapsed since the disco- Since examined by several, but our knowledge of it imperfect.

* Journal des Mines, No. 106, p. 241. The original was published in Crell's Journal, Vol. IV, 1805.

very

Constitution of it in the native state doubted.

very of Scheele. If any one doubt of this, he has only to cast an eye over the different elementary works we have on chemistry, to be convinced of it. Who would not be surprised to see chemists still in doubt respecting the composition of molybdena as it is found native? Some consider it as a sulphuret, in which the molybdena is in the metallic state; while others assert, that they cannot find a particle of sulphur in it, and look upon it as a native molybdena. The smell alone however is sufficient, to convince us of the presence of sulphur in it. Let any one heat laminæ of the purest molybdena, the sulphurous smell, that will exhale, must prove to him, that it contains sulphur, if he have not lost the sense of smell.

Proportion of oxigen in the acid unknown.

Farther we are ignorant of the proportion in which oxigen is combined with the metal to form molybdic acid, though it has been so long known. The want of positive knowledge on these points has led me to think, that, if I were to undertake a series of experiments on molybdena, I should attempt a task of some utility, and that would contribute to augment and improve our knowledge of this substance. To my friend Mr. Haberlé I am indebted for the quantity of molybdena, that has enabled me to make these experiments.

First the existence and quantity of sulphur to be ascertained.

The first thing to be done was, I conceived, to remove all doubt respecting the presence of sulphur, and to determine its quantity. This I imagined would best be effected, by oxigenizing both the sulphur and the molybdena, and separating by means of barytes the sulphuric acid formed. But it was necessary previously to ascertain, whether the molybdic acid, which also forms a salt of no great solubility with barytes, would not occasion some errour in this computation.

I. *Experiments to determine the composition of the native sulphuret of molybdena.*

The native sulphuret contains no excess of sulphur, & no oxigen.

Exp. 1. Twenty-five grains of very pure chosen molybdena were reduced to a fine powder, and heated quickly in a small glass matrass. No sulphur was disengaged. The matrass when cooled contained a little sulphurous acid vapour, and the molybdena, which had been heated red hot, had scarcely

scarcely lost an eighth of a grain. This experiment shows, 1st, that the molybdena contained no excess of sulphur: 2dly, that the heat applied was not sufficient to expel the sulphur from it: 3dly, that there was no oxigen combined with it.

Exp. 2. The molybdena of the preceding experiment was put into half an ounce of pure nitric acid, the specific gravity of which was 1·22, and made to boil on a sand heat. The acid attacked the molybdena pretty briskly, but not so much as I should have supposed. To accelerate the operation, and prevent the sulphur from passing to the state of sulphurous acid, I added a drachm and half of pure muriatic acid, of the weight of 1·135, and a drachm of nitric acid. After boiling for an hour the whole was converted into a homogeneous mass of a milky whiteness, which was diluted with eight times its weight of water; the solution was filtered; and the sulphuric acid, that had been formed, was separated, by washing well both the residuum and the filter. Into the liquor, that had passed through the filter, a solution of muriate of barytes was poured. This occasioned a precipitate, which, being carefully collected, dried, and heated red hot, weighed seventy-two grains, and comported itself as pure sulphate of barytes. To determine the circumstances, in which this precipitate is possible, I made the two following experiments.

Treated with nitric acid, muriatic acid added, and sulphuric acid formed, which was precipitated by barytes.

Exp. 3. Five grains of molybdic acid were mixed with two ounces of distilled water; twenty drops of muriatic acid, of the strength mentioned above, were added; the whole was boiled for half an hour, and the liquor was filtered. The solution had a very rough metallic taste; and solution of muriate of barytes did not render it turbid, though a little sulphuric acid produced this effect immediately.

Molybdic acid not precipitated by muriate of barytes.

Exp. 4. Five grains of molybdic acid and twenty grains of pure liquid ammonia were put into two ounces of water, and the mixture shaken, till the whole was perfectly dissolved. A solution of muriate of barytes being added, a copious flocculent precipitate immediately formed, which was afterward redissolved on adding a few drops of muriatic or nitric acid, and shaking the mixture.

Molybdate of ammonia formed, and muriate of barytes added.

These experiments show: 1, that no indissoluble molybdate

Inferences.

date of barytes is formed, unless the molybdic salt be neutralized; and not when any free muriatic or nitric acid is present. 2, that the molybdena contains a large quantity of sulphur; for the 72 grains of sulphate of barytes, obtained from 25 grains of molybdena in the 2d experiment, represent nearly 24 grains of dry sulphuric acid; which indicate the presence of 10·2 grains of sulphur, or 40·8 per cent. After these preliminary operations I could proceed to a more accurate analysis.

Sulphuret of molybdena treated with aqua regia.

Exp. 5. A hundred grains of laminæ of molybdena, picked with the greatest care, were put into a retort with six drachms of pure muriatic acid, of the spec. grav. of 1·135, and $2\frac{1}{2}$ of nitric acid, equally pure, of the gravity of 1·22, and distilled on a sand bath with a gentle heat. After an hour's ebullition almost the whole of the fluid had passed over into the receiver. What remained in the retort was white, some gray flocks excepted. The liquor was poured back into the retort, half an ounce of fresh nitric acid was added, and when about a third was distilled over the gray flocks disappeared. The liquor that had passed over contained neither sulphuric nor sulphurous acid. The white mass was diluted with six ounces of water and filtered; and the residuum was repeatedly washed with the greatest care.

Precipitated with muriate of barytes.

In order to be well assured, that in precipitating the sulphate of barytes, which I was preparing to do, no molybdate of barytes should be thrown down with it, I added two drachms more of pure muriatic acid to the liquor, which held in solution but a small quantity of molybdic acid. This being done, I added a very pure solution of muriate of barytes, and sulphate of barytes was precipitated. The liquor being passed through a filter previously weighed, the residuum was put into eight ounces of water containing two drachms of muriatic acid, and well shaken; after which it was filtered again, and washed. After being exposed to a red heat it weighed 284 grains, to which 6 grains must be added, as the filter, after being thoroughly dried, had gained so much in weight.

100 grs. sulph. of barytes contain 32·5 of acid: 100 of sulph.

As 100 grains of sulphate of barytes contain 32·5 of sulphuric acid, this quantity must have contained 94·25 grains: and farther, as according to my experiments 100 grains of sulphuric

sulphuric acid contain 42·5 of sulphur, it follows, that 100 grains of sulphuret of molybdena contain 40·56 of sulphur, which differs very little from the result of experiment 2. This agreement seemed to render it unnecessary for me to repeat the analysis: yet certain circumstances, which will appear farther on, induced me to commence a new one, that no doubt might remain. I took a hundred grains of powdered molybdena, put them into a mixture of three ounces of nitric acid and one of muriatic, and treated them as the preceding; with this difference only, that, by employing a larger quantity of acid in the first instance, it was not necessary to return into the retort the liquor that passed over into the receiver. The sulphate of barytes obtained in this instance weighed 288 grains, which, according to the preceding computations, contained 93·6 grains of sulphuric acid, and 39·78 of sulphur. Taking a mean between this result and the preceding, we may conclude, that, 100 parts of sulphuret of molybdena contain

acid, 42·5 of sulph. hence 100 of sulphuret of molybd. contain 40·56 of sulphur.

Experiment repeated.

This gave 39·78 of sulphur.

Sulphur	40
Metallic molybdena*	60

Medium.

Exp. 6. A hundred grains of sulphuret of molybdena, dissolved as the preceding, and distilled to dryness, were put into two ounces of pure liquid ammonia, diluted with an equal quantity of water. The mixture being shaken, in the course of a quarter of an hour the whole was dissolved, except a few yellowish flocks, which, collected on a filter, scarcely weighed a grain after being heated red hot. On boiling this precipitate in muriatic acid it was decomposed, and yielded 0·75 of a grain of silex, with ¼ of a grain of oxide of iron. The smallness of the quantity seems to show, that these two substances were accidentally present in the

Sulphuret of molybdena treated as before, and ammonia added,

about 1 gr. of silex and oxide of iron separated.

* Scheele supposed, that molybdena existed in the acid state in its ore: it was the doctrine of the French chemists, says Fourcroy, that corrected this mistake, by showing Guyton, Pelletier, and all the authors or partizans of the pneumatic theory, that Scheele produced the acid by burning the molybdena, and loading it with as much oxigen, as it could take up. Fourcroy's Chemistry, Sect. VI, art. IV, § 2.

In the subsequent part of this paper of Mr Bucholz it is shown in the most evident manner, that the molybdena is in the metallic state in its ore.

molybdena,

molybdena, and merely in mechanical union with the specimens analysed.

For the subsequent experiments I was desirous of procuring some quantity of molybdic acid. The processes I employed are mentioned in another work, here therefore I shall mention them briefly.

II. *Process for obtaining molybdic acid.*

To obtain the acid, native sulphuret roasted in an inclined crucible.

I took $11\frac{1}{2}$ ounces of native molybdena, the quartz adhering to which was pretty well separated, and put it into a large crucible, which was placed obliquely on the fire. A strong heat was first given, to kindle the sulphur, which was afterward diminished, and the matter was roasted, stirring it occasionally with a wooden spatula. A large quantity of sulphurous acid was evolved, and the mass was entirely covered with a crust of the purest molybdic acid, which was of a lemon colour on the fire, and of the purest silvery white when cold. By taking a little trouble, and using some precautions, the whole might thus have been converted into molybdic acid: but as this would have required a great deal of time and attention, I put an end to the process, when I perceived, that the greater part of the sulphur was volatilized, that a considerable quantity of molybdic acid was formed, and that, on leaving it exposed to a lower degree of heat, the mass began to agglutinate, and even to become fluid near the sides of the crucible. By this operation I obtained $8\frac{1}{2}$ ounces of a gray shining mass, perfectly crystalline, which was of a whitish gray colour when powdered. Half an ounce remained adhering to the sides of the crucible, which could not easily be separated from it.

The acid may be separated by boiling it with an excess of soda, or digesting in ammonia, and precipitating by nitric acid, or expelling the ammonia by heat.

The pure molybdic acid may be separated from the mass by heating it with water, adding carbonate of soda till it ceases to occasion effervescence, and afterward boiling it with a little excess of soda: or the separation may be effected by digesting the mass in pure liquid ammonia, the heterogeneous parts, such as the quartz and oxide of iron, remaining undissolved. On pouring nitric acid into the neutral solution, the molybdic acid is thrown down. The molybdate of ammonia might be decomposed likewise by fire; in which case sometimes

times molybdic acid, sometimes molybdena in the metallic state, or at least approaching to it, is obtained, according to circumstances.

I employed the latter method, that with ammonia, as being the most advantageous. Previous trials had taught me, that three parts of pure liquid ammonia, of the specific gravity of 0·97, dissolve one of molybdic acid reduced to fine powder, and separate it from any impurity, that may be present. In consequence I powdered the produce of the preceding roasting; put it into a bottle with ammonia closely stopped; and left it to digest for twelve hours, shaking it from time to time. The acid disappeared, and two ounces of heterogeneous matter remained, containing still a little molybdena not decomposed. This residuum was boiled with two ounces of common nitric acid, and the molybdena was readily converted into acid, which was obtained perfectly pure by means of ammonia.

The latter method preferable.

3 parts of ammonia dissolve 1 of molybdic acid.

The ammoniacal liquor, in which the roasted mass had been dissolved, became a little turbid at the expiration of five hours, and assumed a yellow ochre colour. Five days after, the matter that occasioned this turbidness had subsided, and comported itself like oxide of iron. Part of the limpid solution was evaporated to dryness, and part of the residuum was heated red hot, to obtain from it the pure molybdic acid, as I had formerly done with a smaller quantity, but my expectation was frustrated. At the beginning the molybdate of ammonia turned blue; and it ended in assuming a metallic aspect throughout, even interiorly; the blue colour changed to a coppery red, and it had a similar appearance to products I had formerly obtained, which every thing indicated to be molybdena in the metallic state, or nearly metallic. The mass became oxided anew on the surface: but it was more agglutinated in those places, where the heat had acted most strongly.

A little oxide of iron precipitated from the molybdate of ammonia.

Effect of heat on the molybdate of ammonia.

III. *Experiments to ascertain the most advantageous method of reducing molybdena to the metallic state.*

Exps. 7 and 8. By the process just mentioned having obtained a mass, which every thing led me to suppose was in the

Attempts to reduce molybdena.

the metallic state, before other experiments had given me farther light respecting its nature, I resolved to employ the disoxigenizing action of ammonia, to obtain metallic molybdena. I took six ounces of liquid molybdate of ammonia, and evaporated to dryness. During the evaporation it diffused a smell much resembling that of vanilla. The saline residuum was pressed tight into a small glass of a convenient form, and covered with a layer of charcoal dust; the glass was bedded in sand in a crucible; and this was set on the fire. After the ammonia was volatilized, the glass was closed with a chalk stopple, the fire urged briskly, and the crucible kept for half an hour in a strong red heat, which melted the glass. After the whole had grown cold, a tolerably compact mass was found, easily reducible to powder, of a copper colour inclining in some places to blue, with a metallic lustre, and exhibiting crystalline laminæ. It weighed three drachms.

Ammonia expelled by heat: residuum covered with charcoal: and exposed to a strong heat. Product.

To see whether this mass would not fuse into a button in a stronger heat, I pounded it; rammed the powder, which was of a violet colour inclining to copper red, tight into a crucible lined with charcoal; covered it with a finger's breadth of charcoal powder; closed the crucible well; and kept it for half an hour at a white heat in a forge fire. After cooling the mass was agglutinated in places where it was most exposed to the action of the fire; but in the middle it was pulverulent, and had retained its colour.

Attempt to fuse it into a button, but merely agglutinated.

Exps. 9 *and* 10. Desirous of repeating the preceding experiments with a greater heat, I put some molybdate of ammonia into a Hessian crucible. After the ammonia had been expelled by a moderate heat, I covered the mass with a stratum of charcoal, closed the crucible, increased the fire to a white heat, and kept the crucible in this state half a hour. After cooling a compact mass was found of a violet brown colour, the lower part of which, that was in contact with the bottom of the crucible, and had consequently experienced a stronger heat, was tolerably consistent; it could not be reduced to powder without difficulty. This powder was of a violet colour, and appeared to consist of a multitude of small crystalline scales of a metallic brilliancy. The fissures that traversed the mass in every direction exhibited on their sides a great

Experiment repeated with a stronger heat. Product.

great quantity of larger scales, likewise of a violet colour, and of a very fine metallic lustre. The outside of the mass was in great part covered with scales also, but smaller, and exhibited a very pretty changeableness of colour. The upper part, which had been in contact with the charcoal dust, displayed some reflections of an indigo blue. The outside of the crucible was spotted with green in several places.

Fire apparently not strong enough.

Several circumstances in the preceding experiment indicate, that the mass would have become more soft and compact, if it had been subjected to a more violent fire. In consequence I took a similar quantity of molybdate of ammonia, and exposed it to the action of the most violent fire for an hour. After cooling I found a mass weighing five drachms in every respect similar to the preceding; except that it appeared a little more compact, and the fissures were filled with scales, which were more crystalline and in larger quantity. These scales, examined with a lens in a strong light, resembled polished false gold; as the largest did when viewed by the naked eye.

Greater heat employed.

Result.

Reasons for supposing the product metallic molybdena:

As the masses obtained in the four preceding experiments had a specific gravity varying from 4·5 to 5·67 according to their different densities, and this is the specific gravity assigned by many to metallic molybdena; as besides, on heating them red hot in contact with air, or with nitric acid, in which case oxigen gas is evolved, they afforded molybdic acid; and lastly as they had a metallic aspect; I was induced to consider them as reguli of molybdena, particularly as no one had ever yet spoken of such an oxide. But subsequent observations taught me, that these masses were molybdena in a peculiar state of oxidation, and that by the processes I had hitherto followed it would be impossible for me to obtain molybdena in the metallic state. It was necessary therefore, that I should attempt its reduction by other means.

but it was a peculiar oxide.

Attempts to reduce it by making into a soft paste with oil.

Exps. 11, 12, *and* 13. I took four drachms of molybdate of ammonia, similar to the two masses obtained in the preceding experiments; powdered it; mixed it with olive oil, so as to make a thick pap; put it into a crucible; and heated it till the oil was burned. It was then pressed down; covered with a stratum of charcoal powder, and over this a little powdered

dered chalk; another crucible was put over it; the fire was urged to a strong white heat, and in this state it was kept for an hour and a quarter. After the combustion of the oil, the mass was pulverulent, of a deep blue colour approaching to black, and in some parts violet: and after it had undergone the strong heat to which it was exposed, it was entirely of an ash gray, and formed a mass of an earthy appearance, the parts of which had but little cohesion; the part in contact with the crucible scarcely showed the slightest indication of fusion; and thrown into nitric acid it produced a more considerable effervescence, than the products of the preceding experiments. The solution thus formed was at first reddish, and afterward became milkwhite. I added concentrated muriatic acid, and boiled to dryness, without any perceptible solution taking place. These circumstances led me to think, that the molybdena was entirely reduced, and that nothing was wanting, but to unite the particles into a button.

Product dissolved in nitric acid, and muriatic added.

To endeavour to form this button, the mass obtained in the preceding experiment, which weighed $3\frac{1}{2}$ drachms, was pressed tight into a small crucible, and exposed anew for an hour and half to the most violent forge fire. This heat was so great, that the whole of the surface was vitrified, and the iron melted and burnt in three minutes. After cooling, the stratum of charcoal powder, with which it had been covered, was scarcely diminished. The molybdena had almost entirely preserved the same form as before; it was of an ash gray colour; its particles were but slightly agglutinated; and no mark of fusion appeared even in the part that adhered to the sides of the crucible. Its weight was $3\frac{1}{2}$ drachms as before.

Attempt to fuse it.

Slightly agglutinated merely.

I took the same mass a third time, powdered it with six grains of charcoal, and exposed it again for an hour and half to a forge fire, which I endeavoured to urge as far as possible. After cooling, the mass had the same ash gray colour as before; on turning the crucible upside down it fell out without breaking; and it had a slight degree of consistency, yet notwithstanding it was friable between the fingers, and easy to pulverize. No mark of fusion could be discovered in the inside of the crucible; but the mass had lost six grains of weight, from part of it adhering to the sides. The mass being

Heated again.

Agglutinated more strongly.

Eagerly ab-

ing thrown into water, this fluid entered its interstices with avidity. sorbed water.

Exps. 14 *and* 15. To know whether the molybdic acid were capable of being reduced by the action of fire alone, without being mixed with any carbonaceous matter, I took a piece of the acid, which had been melted, and weighed 55 grains. This I placed in the midst of charcoal powder in a crucible, and exposed it for an hour and half to the same degree of heat as in the preceding experiment. The result was a tumid mass, which had not any more consistency than that of experiments 12 and 13. It was like that of an ash gray colour, and had lost eighteen grains of its weight, or in the proportion of 32·73 per cent. In nitric acid it comported itself in the same manner as the masses obtained in the preceding experiments. Two hundred and seventy grains of the matter obtained from the molybdate of ammonia roasted in experiments 9 and 10, being treated in the same manner, and kept a quarter of an hour at a white heat, gave a similar product to that of experiments 12 and 13. They had lost 78 grains, or 28·89 per cent.

Molybdic acid exposed to a strong heat imbedded in charcoal.

In a second trial 264 grains of the violet brown oxide, having been kept only half an hour at a moderately strong red heat, the result was a mass but imperfectly reduced. The interior was no way changed; the surface only was gray. Replaced in the fire, and kept at a white heat for half an hour, it was entirely reduced, and lost 74 grains, amounting to 28·03 per cent. Hence it follows, that the substance obtained from the molybdate of ammonia is very far from being in the metallic state.

Attempt to reduce the violet brown oxide.

These experiments show, that oxigen may be separated from molybdena by the action of fire alone when in contact with charcoal. They prove, that the reduction of the oxide and of the acid of molybdena may be effected without great difficulty. It remains to be seen, whether this reduction can be effected with larger quantities, and whether the molybdena cannot be obtained in a button.

Oxigen separable from molybdena in contact with charcoal by heat alone.

Exps. 16 *and* 17. I took ten drachms of molybdate of ammonia; put them into a glass which I placed in a crucible, and exposed to a red heat for half an hour; and obtained

Molybdate of ammonia heated; the brown

 a mass

Oxide bedded in charcoal;

a mass of violet brown oxide weighing one ounce. This mass I placed in a crucible, surrounded it with powdered charcoal, and exposed it for an hour to the most violent forge fire.

and reduced by a violent forge fire.

The result was a metallic mass, the different parts of which were more or less frothy, and more or less tenacious, but the tenacity was in no place such, but it might be beaten to powder. The exterior part was of an ash gray; in the interior, and even at the surface where cavities and depressions were formed, it had a truly metallic lustre, and was of a silver blue colour. The parts that had this lustre being pressed upon and beaten in a porcelain mortar were extended a little under the pestle, and this increased their brilliancy; but on continuing it they were reduced to a gray powder. Their hardness before trituration was greater than that of silver of nine pennyweights (0·75), since they scratched it.

Attempt to obtain it in globules.

In order to obtain this matter in small melted parts, I pounded six drachms, which I pressed as strongly as possible into a crucible lined with charcoal, and exposed to the most violent forge fire for an hour and half. After cooling I found, that the mass was agglutinated, and reduced in bulk one fourth. I could not get it out without breaking the crucible. Those parts which had been in contact with the button and sides had considerable tenacity, but it was not the same with the surface. However, it could not be said to have been actually fused in any place, its parts being merely agglutinated by a commencement of fusion. Every where it exhibited a large quantity of scales, which were of a silvery white and a metallic lustre. Bruised on glass or porcelain, they acquired a medium lustre between that of tin and that of silver; but this disappeared in twelve or fifteen minutes. At the bottom of the crucible appeared a few grains of molybdena, of the size of a pin's head, which had evidently been fluid. They were all over of a metallic lustre, and silvery white, like the scales already mentioned. The lower part of the mass of metallic molybdena, when beaten with a porcelain pestle, assumed the same lustre*.

Agglutinated, but not actually fused.

A few small globules at the bottom, that had been fused.

Notwith-

* Ruprecht appears to have observed something similar. He says, that in endeavouring to reduce molybdena he obtained some small metallic grains

Notwithstanding the molybdena here possessed all the properties, that characterize metals, lustre, compactness, and even malleability, though in a slight degree, I could not obtain a well fused metallic button by exposing afresh to the most violent forge fire for two hours a piece of the mass already obtained, that weighed forty grains. A trial I afterward made with two ounces of brown oxide was attended with better success, than any I had yet obtained. I exposed this oxide to the most violent fire, well kept up, but for one hour only: and though the whole mass was not fused into a button, yet in some parts appeared pieces of one or two drachms, almost entirely fused, having a spherical surface, a white metallic lustre, and a much greater consistency than any mass I had yet obtained. On rubbing these metallic parts against a very smooth piece of porcelain, they assumed a lustre, which it would have been difficult to distinguish from that of silver. I must observe too, that this lustre remained for several days; while in my other trials it did not continue an hour, probably owing to the moisture of the air.

Most successful attempt at fusion.

From the experiments hitherto related we may infer:

General conclusions.

1st, That heat, in decomposing the molybdate of ammonia, causes the acid, in consequence of the disoxigenizing action of the ammonia, to pass to a slighter degree of oxidation, and gives rise to a peculiar oxide, some of the external characters of which have been noticed in the account of the 11th, 12th, and 13th experiments.

2dly, That the oxide and acid of molybdena are completely reducible by the simple action of fire, when they are placed in the midst of powdered charcoal, and that the metal then appears of an ash gray colour: but that, this metal being

grains, the least of which had the appearance of silver; and that the sides of the crucible had a coating of the same colour. He did not venture to assert however, that these grains were entirely metallic; for he observed others which were either of a whitish gray, reddish, or blueish. The following part of this essay, and what has already been said, will show, that these coloured grains belong to the oxide, which has been mentioned. Hielm, observing that molybdena rendered the colour of other metals lighter, inferred, that its own colour was white. This inference is confirmed by my experiments.

difficult

difficult to fuse, the most violent fire must be employed, to obtain a more compact metallic button. The experiments related put the possibility of this out of doubt.

IV. *Determination of the specific gravity of molybdena.*

Specific gravity of molybdena. From the property, which the masses of molybdena I obtained in the metallic state possessed, of imbibing the water in which they are immersed, it is difficult to ascertain their specific gravity with accuracy. In the three trials I made, I suspended the masses by a hair to one of the arms of a very nice pair of scales; and in order to expel the air as much as possible, I boiled them for a quarter of an hour in distilled water. I afterward weighed them at the common temperature.

About 8·611. The first trial gave a specific gravity of 8·636; the second of 8·490; and the third 8·615: we shall not be far from the truth therefore, if we take the mean term of 8·611. It is true the result differs much from that given by some authors, who fix the specific gravity at 4·5, or at 6·5. Hielm gives 7·5 for the maximum: but it is probable, that the masses, with which these were determined, were not pure, or were full of blebs, which occasioned the specific gravity to appear less than it really is to Hielm, Ruprecht, and Heidinger.

V. *Determination of the proportion of oxigen to metal in the molybdic acid.*

Experiment to ascertain proportion of oxigen in molybdic acid. *Exp.* 18. The knowledge of the quantity of metal contained in the native sulphuret of molybdena affords a convenient mode of ascertaining this proportion. For this purpose I took a hundred grains of select scales of molybdena, put them into a small retort with acid as before, and distilled to dryness. Toward the end of the operation sulphuric acid was extricated in gray and heavy vapours. To expel this acid entirely, I broke the retort cautiously; put the pieces into a small glass, which was placed on a sand bath in a crucible; and kept them in a red heat for half an hour. The whole of the sulphuric acid was thus volatilized, and the molybdic acid was left pure in the form of small crystals of a yellowish white colour inclining to gray. This residuum weighed

ninety

ninety grains. The pieces of glass having been weighed, they were carefully washed, and weighed again; when they were found to have lost a grain. Thus the hundred grains of sulphuret of molybdena had yielded ninety-one grains of molybdic acid: and, if we admit as above sixty parts of metal in the sulphuret, the proportion of metal to oxigen will be as sixty to thirty-one, or as 100 to 51·67; consequently 100 parts of acid contain 34·06 of oxigen.

Another experiment.

Exp. 19. Desirous of making a counter trial, I took some of the substance obtained in decomposing molybdate of ammonia by heat, which at that time I considered as molybdena in the metallic state, and endeavoured to oxigenize it. Having poured on it nitric acid of the spec. grav. of 1·22, a brisk effervescence immediately took place, which continued for some time, without requiring the aid of heat; but as I was drying the oxigenized mass, it was suddenly thrown out of the vessel, occasioning a loss, that prevented me from going on with the trial.

This repeated.

I then repeated the operation, employing a very tall glass to contain the oxigenized mass, while I dried and melted it. A hundred grains, treated with ten drachms of nitric acid, produced a radiated mass, that weighed exactly a hundred and nine grains; and, if we add one grain for what may have remained adhering to the vessels employed, we shall have 110 grains of acid from 100 of the metallic substance.

The substance obtained by expelling the ammonia from the molybdate was an oxide.

This result differed so widely from the preceding as to convince me, either that the substance I had taken for molybdena in the metallic state was not actually so, or that I had made some mistake in determining the quantity of sulphur contained, and the quantity of oxigen absorbed, by the native sulphuret of molybdena. I resolved therefore to repeat my experiments.

Experiment with the sulphuret of molybdena.

I have already given the result of the second experiment I made to find the quantity of sulphur contained in the native sulphuret. What I did to verify the proportion of oxigen to metal in the formation of the acid consisted in taking a hundred grains of native sulphuret of molybdena, which I put into a mixture of one ounce of muriatic acid and three ounces of nitric acid; and, in order to prevent loss from any being

being thrown out, I conducted the oxigenation in a tall vessel, which I placed first on sand, and afterward in a crucible, the sides and bottom of which were coated with chalk. By this process I obtained ninety grains of molybdic acid, which indicate fifty parts of oxigen to a hundred of metal; so that 100 parts of molybdic acid would consist of 66·67 metal and 33·33 oxigen.

The regulus of molybdena, which I had obtained in my preceding experiments, afforded me another mean of verifying the results.

Experiment by acidifying the metal itself.

Exp. 20. A hundred grains of the metallic molybdena of experiment 13 were reduced to very fine powder, put into a porcelain capsule, and thirteen drachms of pure nitric acid added. An extraordinary effervescence took place, and a great deal of nitrous gas was evolved. On evaporating, the matter, which was at first of a brownish yellow, passed gradually to a whitish yellow. In drying it became orange, and even blue in those places where the heat was the strongest. After it was well dried, and collected together, it was fused in a glass; and its weight was found to be increased thirty-four grains, which indicates 25·37 parts of oxigen in 100 of molybdic acid. It was thoroughly crystalline, and formed crystals of a silver white inclining to gray.

Variation in the proportion of oxigen indicated.

Was it owing to charcoal?

The change of colour just mentioned, which has not been remarked before, indicated a variation in the proportion of oxigen to metal. It appeared to me probable, that a portion, though small, of the charcoal, which had been mingled with the molybdena to promote its reduction, had combined with it, produced the phenomena observed during the oxigenation, and changed the proportion of the oxigen to the metal.

Two attempts to ascertain this frustrated.

Exp. 21. To verify this suspicion, I thought it necessary to repeat the trial, employing molybdena that I had reduced by simply placing the mass to be reduced in the midst of powdered charcoal, without having triturated and mixed them together. The experiment failed twice. The first time the effervescence and swelling up on pouring the nitric acid on it were so great, that the matter ran over the sides of the vessel: and the second, though the acid had been diluted,

luted, explosions took place, by which some of the molybdic acid was thrown about.

To prevent these accidents, I made the trial again in wide-mouthed capsules. I took a hundred grains of powdered metal, and poured on it an ounce of nitric acid diluted with half an ounce of water. In a few minutes a very powerful action took place, and the liquor became of a yellowish red inclining to brown. The whole of the metal not being dissolved, when no more gas was evolved I added half an ounce of acid, and placed the solution on a sand bath. The whole was now dissolved, but the liquor remained of a yellowish red inclining to brown as before, only a reddish white powder appeared swimming in it. I evaporated the mixture to dryness, stirring it constantly. The residuum had a red copper colour, mixed with a great deal of white; on continuing the heat the surface acquired a grayish blue, the edges a brown red, and in some parts an orange yellow. A third

Thus it was evident, that the difference of colour exhibited by the oxigenized molybdena was not occasioned by charcoal mixed with it, but might be ascribed to different degrees of oxidation of the metal. It is surprising, that the molybdena should become oxigenized so imperfectly in this manner; while its oxigenation is accomplished much more speedily and completely, when sulphuret of molybdena is employed. To produce a perfect oxigenization, I poured half an ounce of nitric acid on the dry mass, and heated it. Finding no perceptible change take place, I added two drachms of pure muriatic acid, which expedited the effect, the mass became more and more compact and lighter coloured, and at length was white. Being dried and carefully collected, it was kept at a red heat in a glass capsule for a quarter of an hour, left to cool, and weighed. Its weight was now 145 grains, to which must be added three for what adhered to the sides of the capsule, so that in this experiment a hundred grains of molybdena had absorbed 48 grains of oxigen. In 100 parts of acid therefore there are 32·43 of oxigen.

showed that charcoal was not concerned in it.

The sulphuret more easily acidified than the metal.

This experiment then gives a result differing little from those obtained in the 14th and 18th. I repeated the first once more. Taking a piece of fused molybdic acid, that weighed

Exp. 14 repeated.

weighed 100 grains, I put it into a crucible in the midst of powdered charcoal, and exposed it for an hour to the most violent forge fire. The gray mass I obtained weighed exactly 32 grains less than the acid employed.

Molybdic acid contains about 67·5 of metal, and 32·5 of oxigen.

Thus we may admit, without being far from the truth, that a hundred parts of metallic molybdena absorb forty-nine or fifty parts of oxigen, when this metal passes to the acid state, and that consequently a hundred parts of molybdic acid contain thirty-two or thirty-three of oxigen.

This confirms the proportions of the sulphuret.

These experiments on molybdena in the metallic state, and on molybdic acid, confirm the proportions I have assigned to the constituent parts of the native sulphuret of molybdena.

(To be continued.)

XI.

Construction of a Curb affording a Compensation for the Effects of Heat and Cold in Time-pieces. By Mr. WILLIAM HARDY, *No. 29, Cold-bath Square.*

Compensation curb described.

IN Pl. IV, fig. 2, the circles A B denote an expansion bar, composed externally of brass and internally of steel; in consequence of which its curvature will be diminished by heat, and increased by cold.

Manner in which it acts.

The end A is fixed, and the rest of the bar is at liberty, whence the extremity B will be affected by a motion from temperature, which will carry the pin F (between which and G the spring D E passes) a little onwards when the temperature rises, and a little backwards when it falls. But F will be scarcely at all affected in the direction of the radius, because the two semicircular parts of the bar counteract each other. C G is an arm of brass fixed to the expansion bar, in the middle of its length. It carries a pin G opposite to F; and as C is thrown inwards by increase of temperature through a space, amounting to the whole change to which the diameter of the curved expansion bar is liable, G will be carried towards F, and will allow the spring less play; and by that means add to its stiffness,

The balance

stiffness, when its elastic force is diminished by the weather: and the contrary effect will take place when the spring becomes more rigid by cold. spring will have less play in hot weather.

An adjustment for temperature has been applied in chronometers several years ago, by means of a pair of tongs, or double expansion bar, of the same nature as if the spring were to pass through the variable notch A B in the present figure. Whether Mr. Hardy's contrivance will afford superior advantages in its effect, or in the convenience of disposing the parts, must be referred, like all things of this nature, to trial. A former adjustment on a similar principle.

W. N.

XII.

Of the Yenite, a new Mineral Substance: by Mr. LE LIEVRE, *Member of the Institute, Counsellor of Mines, &c.**

WHEN I was sent to the isle of Elba five years ago as commissary of the government, I thought I might avail myself of the opportunity, to study and make known the mineralogy of a country so interesting to the naturalist. The business of my office, however, having occupied nearly the whole time I spent in that island, did not allow me to carry this design into execution: but my journey will not have proved altogether fruitless to the science of mineralogy; for, beside the mineral that forms the subject of the present paper, I have brought with me some others, that may prove interesting to the mineralogist. Such are Minerals from the isle of Elba.

1. A green substance, that has some resemblance to actinote, and a considerable analogy to that which I am about to describe. A new mineral.

2. Transparent white emeralds, some of which are three centimetres [1·18 inch] long. White emeralds.

3. Black, yellow, and rose-coloured tourmalins. Tourmalins.

4. Rose-coloured and white lepidolite, lamellar and compact. Lepidolite.

* Journal des Mines, No. 21, p. 65. Extracted from a paper read at the meeting of the Institute, December the 29th, 1806.

5. A

Porphyry. 5. A porphyry with base of white compact feldspar, and containing black globular nodules, which appear to me to be a mixture of amphibole and feldspar.

Diallage. 6. Green and metalloïd diallage.

Resinite. 7. Resinite quartz similar to that of Musinet in Piedmont.

Fetid quartz. 8. Fetid pseudomorphous quartz, &c.

In some future papers I shall mention what I have noticed, that is peculiar, either in the characters or situations of these: but I shall confine myself for the present to that, which I now lay before the class, and to which I have given the name of *yénite*, from of one of the most memorable events of the age, the battle of Jena. Yenite.

Its physical characters.

Spec. grav. This mineral weighs nearly four times as much as distilled water (3·825, 3·974, 3·985, 4·061).

Hardness. Its hardness is a little inferior to that of the adularia feldspar, by which it is scratched; but it scratches glass strongly, and gives a few sparks with steel.

Primitive form. The mechanical division leads, as will be more particularly described presently, to a rhomboidal prism of 113° and 67°, which may be subdivided parallel to the shorter diagonals of its bases.

Colour. The yenite is opake, and of a black colour inclining sometimes to brown. Its powder is of the same hue.

Surface. The surface of the crystals, when they are very black, is shining. (Those varieties represented Pl. IV, figs. 5 and 6, have commonly a dull and brownish surface.) The lateral faces of the prisms are streited lengthwise: the facets O of the summit are smooth and very shining.

Fracture. The fracture is unequal, and of a greasy lustre (nearly like that of phosphate of manganese).

Nonelectric. It is not electric, either by heat or friction.

Magnetic when heated. Heated to redness in the flame of a wax candle merely, it becomes weakly attractable by the magnet.

Spontaneously decomposed. Exposed to the action of the air it is decomposed, and covered with an earthy yellow and brown crust, perfectly similar to the ochres, or oxides of iron mingled with earths, which are found native.

Geometrical

Geometrical characters *.

Cleaving exhibits indications of laminæ parallel to the sides of a prism with a rhombic base, the angles of which are 112° 37′ 9″ and 67° 22′ 51″. Rather more evident indications are found of a division according to the shorter diagonals of the rhombs. This section is pointed out on the crystals by the striæ at the summit. The bases present no section: their fracture on the contrary is uneven conchoidal, &c. Mechanical division.

The primitive form, Pl. IV, fig. 3, is a right prism, with a rhomboidal base, the diagonals of which are to each other as 2 to 3. From the theory of decrements its height is to the shorter diagonal as 4 to $\sqrt{7}$. Primitive form.

The crystals have five varieties with respect to figure. Figures of the crystals.

Var. 1. Fig. 4 is the primitive form elongated, and terminated by a pyramid with four faces rising from its edges. The angle of incidence between M and O is 128° 28′ 59″; that between O and O, 139° 36′ 48″; and that between O and its reverse 117° 38′ 8″.

Var. 2. Fig. 5 is a tetraedral prism, nearly rectangular, terminated by a double bevil, obtuse, and placed on the obtuse angles. The angle of incidence between S and S is 83° 16′ 4″; that between R and its opposite face 113° 2′ 9″.

Var. 3. Fig. 6 is the preceding form with a double truncature at each acute angle of the bevil. The angle of incidence between O and R is 159° 48′ 24″.

Var. 4. Fig. 7 is an octaedral prism terminated by an obtuse octaedral summit, four of the faces of which are at the angles of the prism, and four on the edges. The angle of incidence between X and the edge Z is 131° 24′ 37″.

Var. 5. Fig. 8 exhibits the preceding variety with this difference, that it has at the summit a facet parallel to the base of the primitive form. The angle of incidence between P and R is 146° 31′ 43″; and that between P and O, 141° 31′ 1″.

* These characters were ascertained by Mr. Cordier, engineer of mines, who was so obliging as to undertake the examination of the crystalline forms, which he calculated according to the method of our learned comrade Haüy.

At

Differs from all others.

At first view this mineral seems to approach the epidote in form: but in the first place the regularity of the faces forms an objection; and in the second, the measure of the angles, and the laws of decrement, totally contradict this apparent analogy. No other mineral substance has any similarity with this new species, at least as far as respects form.

Chemical characters.

Chemical characters.

The yenite simply calcined becomes attractable by the magnet, passes from black to a very dark reddish brown, and loses about two per cent of its weight.

It readily fuses before the blowpipe, without any sensible ebullition; and yields an opake, black button, very readily attracted by the magnet, but without polarity, dull, and exhibiting a metallic aspect. With glass of borax it dissolves with a short effervescence. On continuing the fire an enamel is obtained, that appears black: if a larger quantity of borax be added, we have a transparent glass, of a yellowish green colour, without any indication of a metallic button, or residuum; which proves, that the whole has been dissolved.

It is attacked by the sulphuric, nitric, and muriatic acids. The last dissolves it most readily: the silex remains at the bottom, and the solution acquires a fine yellow colour, with a slight tinge of green.

Analysis.

It has been analysed by Messrs. Vauquelin and Descotils, and a hundred parts gave

Component parts.

	Descotils.	*Vauquelin.*	
Silex	28	29	30
Lime	12	12	12·5
Oxide of iron	55	57	57·5
Oxide of manganese	3		
Alumine	0·6		
Loss	1·4	2	
	100.	100.	100.

This

This agreement between the results obtained by these two skilful chemists, who operated on the stone at the same time, and unknown to each other, gives these analyses as high a degree of certainty as could be wished; and authorises us to conclude, that the yenite, at least in those specimens analysed, contains rather more than half its weight of iron, mixed with a little manganese, and that the rest of the stone is lime and silex, the proportion of silex being considerably more than double that of the lime.

Situation and local circumstances.

I found the yenite in two different places in the island of Elba; at Rio-la-Marine, and at Cape Calamite. Where found.

In the first of these it forms part of a very thick mass or stratum, resting on a primitive limestone mixed with talc (a kind of *cipolin* marble); the whole exhibiting a cliff, or bare perpendicular rock, about thirty yards high. It is imbedded in the green substance, which I have mentioned as bearing considerable analogy to it, in masses that reach to the size of a few cubic decimetres [the dec. is near 4 inches], and frequently form the sides of cracks in the rock. These masses are most frequently composed of distinct pieces, and in each of these pieces the mineral is in radii diverging from a centre. Sometimes the radii are nearly parallel, and so conjoined together, as to exhibit compact masses, which divide into shapeless prisms like certain basaltes. At other times the radii, particularly when their extremities are free, terminate in true crystals. Frequently the yenite appears in long pieces, or imperfect prisms, of the size of a man's finger, and sometimes even much more slender, in the midst of the green substance; from which it is very distinct, their limits being always decidedly marked. Frequently too it is found in cavities of this substance, in crystals sometimes with a polyedral summit at each extremity, and 3 or even 4 cent. [1 inch or $1\frac{1}{2}$] long; sometimes they are solitary, at others variously grouped. The stratum that has been mentioned includes likewise epidote of a fine yellowish green, quartz, some crystals of arsenical iron, and that variety of amorphous oxidulated iron called loadstone. Its situation in the earth.

At Cape Calamite the yenite is found in the same substance,

stance, but of a grayer colour, and of an aspect similar to that of certain asbestiform actinotes. It is accompanied by oxidulated iron, garnets, and hyaline quartz*.

An old specimen said to have come from Siberia.

I have lately examined a specimen in my mineralogical collection, which I have had several years, and which, not being able to refer it to any of the known minerals, I had put into a particular place, as is my custom, for farther examination. This specimen is black yenite, imbedded and as it were disseminated in the same greenish substance. It is accompanied with a note, that marks the part of Siberia between Perm and Tobolsk for its native place. I cannot venture however to warrant the authenticity of this indication.

The substance of which I have given the history might perhaps be employed as an iron ore, and smelted for its metal, if it were more abundant than I have yet observed, and not so near one of the richest iron mines in Europe.

Always accompanied by a green mineral resembling strahlstein.

It has been seen, that the yenite, whether at Rio, at Cape Calamite, or in Siberia, is always accompanied with a green substance, disposed in fibres or rays like actinote. To this geological relation may be added, that there is a much closer in their composition: these two minerals differ only in this,

Found nine years ago.

* Mr. Fleuriau de Bellevue, to whom I showed the specimens of yenite I had, telling him I considered it as a new substance, informed me, that he himself had brought home specimens of the same mineral from Cape Calamite nine years ago, and that it was analysed by Mr. Vauquelin the year following. He at that time obtained from it

Its analysis as then given.

Silex	30
Lime	14·8
Oxide of iron	49
Oxide of manganese	2
Alumine	1
	96·8

In Romé de Lisle's collection.

Since my memoir was read, Mr. Gillet-Laumont has found in the collection of Romé de Lisle, now in his possession, crystals of the same substance; and informs me, that they were placed by that learned mineralogist at the end of the tin ores. I believe I may venture to assert, that they came from Rio. This mineral was at Paris therefore long before it was brought thither by Mr. Fleuriau de Bellevue, though it was not known.

that

that one contains a much larger proportion of iron than the other. They have besides almost all the same physical and chemical characters; whence I am induced to consider them as forming but one species. In a subsequent paper on the green substance I shall give more at large the reasons, by which I am led to this opinion; and I shall point out the place, which I conceive they ought to occupy in the classification of minerals.

XIII.

Memoir on the reciprocal Action of several Volatile Oils and certain Saline Substances: by Mr. MARGUERON, *late Apothecary-major to the Hôtel des Invalides*.*

I Inserted in the 21st vol. of the Annales de Chimie a paper containing some results of the action of cold on volatile oils, and an examination of the concretions found in several of these oils. The object of the present is, to make known the reciprocal action of several saline substances with oils of that kind, and to point out the alterations such salts are capable of producing in them. Object of the author.

Exp. 1. I made a saturated solution of acetite of lead in distilled water at 15° [59° Fahr.], divided this solution equally in four phials, and added to each portion one eighth of its weight of the following volatile oils; namely those of thyme, lavender, rosemary, and sage. I shook each phial, till the oil in it was broken into globules, that the contact might be the more intimate. After keeping these mixtures several months, the following were the results. Solution of acetite of lead with volatile oils

The oil of thyme had undergone no alteration; but the part in contact with the solution contained several whitish bladders, which at the slightest motion separated in films. The oil being afterward filtered differed in no respect from what it was at first. of thyme,

The oils of rosemary, lavender, and sage, likewise expe- rosemary, la-

* Annales de Chimie, vol. XLVII, p. 46.

vender, and sage.

rienced no alteration; and no flocks formed in them, as in the oil of thyme. I filtered these oils, as I did the preceding, and assured myself, that they had not been altered by the solution of acetite of lead, which remained very clear. Into each of these oils I dropped a few drops of sulphuret of potash, which occasioned no precipitate and produced no colour.

Sulphate of alumine with oils of lavender, sage, hyssop, and rosemary.

Exp. 2. I mixed eight parts of a cold saturated solution of sulphate of alumine with one part of the volatile oils of lavender, sage, hyssop, and rosemary, each separately. These mixtures having been kept four months in flint glass phials with ground stopples, neither the oils nor the solution of alum had undergone any change.

Muriate of lime with oil of the vulnerary plants.

Exp. 3. I mixed eight parts of solution of muriate of lime with one part of the volatile oil of the vulnerary plants in a phial, and kept the mixture for a month, shaking it now and then, without perceiving the slightest alteration. In this state I added liquid potash, to decompose the calcareous muriate; but I merely found, that the oil had evidently lost some of its colour, and become whiter.

Oil of lemons and sal ammoniac.

Exp. 4. Volatile oil of lemons, expressed from the rind, being mixed with a solution of sal ammonia, and kept for a month, shaking it frequently, underwent no change.

Hyperoximuriate of potash with oils of thyme, lavender, peppermint, lemons, and cloves.

Exp. 5. Into five phials I put a solution of superoxigenized muriate of potash, made in distilled water at a temperature of about 15° [59° F.]. In one I added an eighth part of oil of thyme; in the second, an eighth of oil of lavender; in the third, an eighth of oil of peppermint; in the fourth, an eighth of oil of lemons; in the fifth, an eighth of oil of cloves. I put the bottles into a place secluded from the light, and kept them thus a month, shaking them once every day. Neither the oils nor the solution of the salt experienced any alteration. I then placed the five phials in a water bath, which I heated so as to make the water boil for a moment. These oils retained their colour, smell, fluidity, and transparency; and all of them their property of floating on water, except the oil of cloves, which sunk in it as usual. I separated each of the oils from the saline solution, evaporated each portion of the solution separately,

separately, and obtained by crystallization the superoxigenized muriate of potash just as it had been before it was dissolved.

Exp. 6. Into a phial I put two parts of recently made lime-water and one part of volatile oil of rosemary. The mixture became white on shaking it: but on standing the oil separated as fluid, and as transparent, as before, though whiter. The portion in contact with the lime-water formed a light whitish coagulum: and the lime-water had acquired a dull yellow colour, without having lost its property of being precipitable by oxalic or carbonic acid. Lime-water and oil of rosemary.

Exp. 7. Crystals of nitrate of mercury, obtained from a solution of the metal made without heat in nitric acid, were enclosed in a phial with four times their weight of oil of rosemary, and immediately a curved tube, terminating under a glass jar filled with water, was inserted into the mouth of the phial. I shook the mixture occasionally, leaving it thus for six days. I perceived no commotion, or evolution of gas; the oil acquired an amber colour; the quantity of crystals of nitrate of mercury was considerably diminished; and I observed a gray powder, among which globules of fluid mercury might be distinguished. Having poured the contents of the phial on a filter, the oil passed of a reddish colour, with the thickness of a fixed oil, and with an empyreumatic acid smell, that was predominant over that of rosemary. It contained likewise a small portion of nitrate of mercury in solution. Copper was whitened by it. Nitrate of mercury with oil of rosemary,

The mercurial matter remaining on the filter was gray, glutinous, and intermixed with globules of mercury. Spirit of wine passed through it acquired a reddish colour, and grew milky on the addition of water, like a resinous tincture. After this washing with spirit of wine a gray oxide remained, with which globules of mercury were mingled. and of lavender.

This experiment repeated with volatile oil of lavender afforded the same results.

Exp. 8. I prepared a solution of corrosive sublimate in the proportion of ten grains of the salt to an ounce of distilled water, for a series of experiments, which I shall here relate. Solution of corrosive muriate of mercury

1st. Into a phial I put one ounce of this solution and some with oil of lemons,

some volatile oil of lemons recently rectified, and perfectly colourless. I shook this mixture from time to time. In ten days the oil had subsided to the bottom in globules of a light amber colour, which, when separated by filtration, dissolved in spirit of wine. This solution being mixed with distilled water, the oil reappeared, with the property of swimming on the surface of water, being divested of the mercurial salt, which it had dissolved. The solution of corrosive sublimate separated from this oil had a somewhat acid smell, not unlike that of the residuum left after rectifying oil of lemons, and formed a yellow precipitate with lime-water as usual.

oil of chervil, 2d. Volatile oil of chervil, that had been made two years, treated with a solution of corrosive sublimate, was likewise precipitated in the form of globules, without its colour being lightened. I separated the oil in the same manner by the filter, and dissolved it in spirit of wine. Water mixed with this solution did not free the oil from the corrosive sublimate it had dissolved, for it still remained heavier than the fluid, and kept at the bottom of the vessel.

oil of hyssop, 3d. Volatile oil of hyssop, that had been long made, treated with the same solution of corrosive sublimate, sunk to the bottom of the liquid at the end of four days, without any change of colour.

oil of peppermint, 4th. Oil of peppermint, recently distilled, with the same reagent thickened, became greener, was precipitated, and adhered to the sides of the phial. The solution of this oil in spirit of wine was of an emerald green; and the addition of water caused the oil to make its reappearance with its natural green colour.

and oil of lavender. 5th. Oil of lavender, agitated in a similar solution of corrosive sublimate, was precipitated at the expiration of a few days, and became of a high amber colour approaching to red. On the sides of the phial a whitish mercurial crust was observed.

This oil, when separated from the solution, had lost its fluidity; its smell was considerably changed, being acid and empyreumatic; its colour was reddish; and it spotted glass in the manner of empyreumatic oils. Beaten up with distilled water, it first subsided to the bottom, and after some time

time rose to the surface. Shaken afterward with lime-water, it gave signs of the presence of corrosive sublimate by the yellow precipitate it formed. It dissolved completely in spirit of wine: but on the addition of water it reappeared, in part light, in part heavy. I kept some of the solution of this oil in alcohol a considerable time: and I observed, that the sides of the phial acquired a white coating, which I found to be mild muriate of mercury. The solution of corrosive sublimate separated from the oil was not entirely decomposed, it still manifesting the presence of this salt by the yellow precipitate it formed with lime water.

Crystals of corrosive muriate of mercury with oil of rosemary.

Exp. 9. Into a phial I put forty grains of corrosive sublimate crystallized from water, and poured on them an ounce of volatile oil of rosemary. In the course of a few days it acquired a deeper amber colour than it had before, and a white flocculent precipitate formed in it. I then added more corrosive sublimate, which changed the colour of the oil to a very deep green; the precipitate from white became green; the oil lost its fluidity; and it emitted an empyreumatic acid smell, with which that of rosemary was faintly perceptible. I separated the oil from the precipitate, mixed it with water, and heated the mixture till it boiled. The oil did not change its colour, and still remained heavy. I washed it several times with distilled water, which freed it from the mercurial salt it had dissolved; it again became light enough to swim on the surface of water; it spotted the glass in the manner of empyreumatic oils; and its solution in spirit of wine was greenish. The green precipitate mentioned above had a strong smell of rosemary, and burned with a vivid flame. On adding spirit of wine to it, the resinous part was dissolved, and formed a green tincture, which turned white on the addition of water, in the same manner as resinous tinctures. That part of the precipitate, which remained insoluble, was mild muriate of mercury mixed with a little corrosive sublimate.

Oil of turpentine with powdered muriate of mercury, and with its solution.

Exp. 10. Corrosive sublimate in powder, shaken with oil of turpentine rectified from water, had no effect on this oil, though I kept the mixture a long time. The solution of this salt, on the contrary, after a certain time produced a change; causing it to assume the consistence of turpentine,

while

while mild muriate of mercury was deposited on the sides of the phial.

Calomel and oil of lavender.

Exp. 11. Oil of lavender kept several months on mild muriate of mercury, well washed and well levigated, underwent no alteration.

Cinnabar and oil of rosemary.

Exp. 12. Factitious cinnabar in powder and oil of rosemary exhibited no reaction during or after their mixture.

Sulphate of mercury and oil of rosemary.

Exp. 13. Oil of rosemary poured on turbith mineral, and kept some time, experienced a change. It let fall a greenish flocculent precipitate, and at the same time acquired the property of sinking in water, when poured on it. A portion of the turbith was converted into gray oxide of mercury, which retained in combination that part of the oil, that had become resinous.

Red nitrous oxide of mercury and oil of lavender, in the shade;

Exp. 14. I put into a phial a drachm of red precipitate with one ounce of volatile oil of lavender, and kept the mixture several months in the shade. The oil had formed a whitish sediment, and the red precipitate was in part reduced to gray oxide of mercury. The whitish sediment was soluble in spirit of wine, and this solution turned milky on the addition of water. The oil retained the fluidity, colour, and smell of oil of lavender, swam on water, and dissolved well in alcohol.

in the sun.

Having exposed a similar mixture to the light of the sun, the oil lost somewhat of its transparency and deposited a whitish sediment, and the red precipitate had assumed the shining metallic colour of iron filings.

Muriate of antimony and oil of rosemary equal parts.

Exp. 15. Into a small phial I put a drachm of crystallized caustic muriate of antimony, and poured on it an equal weight of oil of rosemary. On corking the phial I immediately perceived a great heat, the cork flew out, the oil spouted out with violence, and the inside of the phial remained coated with a black oil of a particular smell mixed with the smell of rosemary.

This mischance induced me to repeat the experiment in a larger vessel, and with different proportions of the ingredients.

8 parts of the oil to 1 of the muriate.

I put into a phial eight parts by weight of oil of rosemary with one of crystallized caustic muriate of antimony. The heat was scarcely perceptible; the muriate of antimony fell

fell to pieces very slowly; and the portion of oil that was in contact with it acquired a deep amber colour. In a few days the whole of the oil was turbid, and exhibited interiorly a flocculent precipitate. As soon as the oil appeared to be grown clear, I poured it out on a filter, on which a light orange coloured matter remained, that I shall notice presently. The oil passed through of a deep amber colour; it had the consistency of an expressed oil; and its smell was less sweet. Being kept a long while in a phial, it deposited a whitish sediment. Poured into distilled water, and gently shaken, it separated into flakes, which rose to the surface of the water, and let fall a silky precipitate, of a very white and silvery appearance. This was insoluble both in water and in alcohol, was not volatilized at a red heat, and resembled the silvery flowers of antimony. The oil dissolved easily in alcohol, separated from it on the addition of water, and let fall on standing a precipitate similar to that abovementioned. The orange coloured matter left on the filter, having been washed repeatedly with spirit of wine, and dried on paper, exhibited a crystalline appearance in certain parts. Put on a slip of iron, and heated to redness, it became covered with shining needles, as muriate of antimony does.

Nitrate of silver and oil of rosemary.

Exp. 16. Oil of rosemary added to a solution of nitrate of silver exhibited after some time a whitish pellicle, which had a metallic aspect, and formed a separation between the solution and the oil. The oil did not appear to be altered, and the solution of nitrate of silver still formed muriate of silver on the addition of muriatic acid. The pellicle was a portion of the silver reduced to the metallic state.

Volatile oils with sugar.

Exp. 17. The volatile oils in the experiments lately mentioned having undergone a great deal of alteration, I thought it right, to continue the examination of the same oils with a less active substance, as sugar, with which they are supposed to be capable of forming a perfect combination, completely soluble in water, without losing any thing of their properties. Such a compound is called an *oleosaccharum*, and is used to impart to different preparations, solid or liquid, the smell of a fruit, flower, or some other part of a vegetable.

vegetable. Whatever be the proportion of sugar and volatile oil directed in different Dispensatories, the authors are agreed on the intimate nature of the union; and conceive, that this method renders volatile oils miscible with aqueous liquors.

For an oleosaccharum the oil should be highly rectified.

Volatile oils reduced to an oleosaccharum are the more easy to unite with aqueous liquors, in proportion as the oils are more fluid, and more highly rectified. When they are so to a sufficient degree, they even dissolve in a large quantity of water alone. Mr. Baumé remarks, in his Elements of Pharmacy, that essential oils, when they quit a vegetable to rise in distillation with water, undergo even in this a true rectification. That which rises first with the milky water is more fluid and more fragrant, than what passes after the receiver has been changed. The latter does not whiten the water: the former dissolves in it in consequence of its tenuity, and gives it that milky whiteness, which is observed as long as it passes over in the distillation. Accordingly an oleosaccharum made with these first oils is agreeably aromatic, does not render the aqueous liquor turbid, and does not separate from it: while on the contrary the second, of which we have been speaking, or any other volatile oil that has been made a certain time, or a mixture of both these oils recently made, forms an oleosaccharum that is not of a pleasing smell or taste, renders the aqueous mixture turbid, and separates from it in a very short time. We may ascertain this fact, by putting into a glass tube filled with water an oleosaccharum made with this oil. It will fall to the bottom; the air contained in the sugar will be extricated; the oil will rise through the fluid in small globules and collect on the surface; if the mixture be shaken it will turn milky, and be a long while becoming clear again; but the oil will at length reappear, having experienced a sort of thickening, so that instead of globules it will form a sort of mucilaginous flakes. The oleosaccharum then is an imperfect combination, when it is prepared with an oil not sufficiently rectified. It is to be observed, that many lozenges made by baking, and flavoured with essential oils, as those of peppermint, anise, &c., are oleosaccharums

The best oil rises first.

The combination of the oil with the sugar

saccharums that do not let the oil separate on solution in water, though commonly the makers are indifferent about employing highly rectified oils; not to mention, that the most volatile part of the oil must be dissipated by the heat, to which it is exposed during the baking of the sugar. It would seem, that in this case the heat must produce a more intimate combination with the oil, the nature of which it would be interesting to know.

promoted by baking.

Exp. 18. As I had at hand a certain number of essential oils, I thought it not amiss to make a series of experiments, for the purpose of ascertaining the heat or cold they would produce when shaken with water. These experiments furnished me with a new mode of detecting those adulterated by a mixture of spirit of wine.

Experiments to find whether heat or cold be produced on mixing oils with water.

1. I mixed volatile oil of peppermint with thrice its weight of distilled water, and plunged a mercurial thermometer into the mixture. The temperature was 10° [50° F.], and no change took place.

Oil of peppermint. No change.

2. Common oil of peppermint of the shops, mixed with water in the same proportion, at the same temperature raised the thermometer $1\frac{1}{2}°$ [2·7°].

Common oil of peppermint. Heat.

3. Oil of lemons recently rectified from water on a water bath, as limpid and as fluid as ether, being shaken with distilled water, produced neither heat nor cold.

Oil of lemons.

4. Oil of orange flowers recently distilled comported itself in the same manner; while the orange flower oil of the shops with the same quantity of water raised the thermometer 1° [1·8°].

Oil of orange flowers.

These experiments having taught me, that some essential oils produce neither heat nor cold by their mixture with water; while others on the contrary produce a sensible heat, though operating with very small quantities; I conceived, that the cause of this heat was ascribable to spirit of wine, employed to adulterate them. Accordingly I mixed one part of spirit of wine with two of an essential oil, kept the mixture some time, and then mixed it with thrice its weight of water, when it caused the thermometer to rise 1° [1·8°].

When heat is produced, they are adulterated with spirit of wine.

General

General conclusions.

General results.

From the experiments above related it follows:

Effects of acetate of lead, or of alum; 1. That the oils of thyme, lavender, rosemary, sage, and lemons, undergo no alteration, even by standing, with solution of acetate of lead, or of alum.

of muriate of lime; 2. That the oil of the vulnerary plants with a solution of muriate of lime, loses its lemon yellow colour, and becomes whiter.

of hyperoxi-muriate of potash; 3. That the solution of superoxigenized muriate of potash occasions no change in the oils of thyme, lavender, peppermint, lemons, or cloves.

of lime water; 4. That lime-water destroys in part the colour of oil of rosemary.

of nitrate of mercury; 5. That nitrate of mercury is decomposed in oil of rosemary, which it renders high coloured.

of corrosive sublimate; 6. That corrosive sublimate, and its solution in distilled water, increase the colour and consistency of the oils of lemons, chervil, hyssop, lavender, and rosemary, and are partly decomposed by them, producing mild muriate of mercury.

of calomel and cinnabar; 7. That mild muriate of mercury and factitious cinnabar occasion neither action nor reaction with oils of lavender and rosemary.

of turbith mineral; 8. That turbith mineral is partly decomposed in oil of rosemary.

of red precipitate; 9. That red precipitated mercury is in part converted into gray oxide in oil of lavender, without causing the oil to undergo the least alteration.

of muriate of antimony; 10. That caustic muriate of antimony is decomposed in oil of rosemary, which it colours and thickens; while part of this muriate loses its acid, and appears to be converted into silvery flowers of antimony.

of sugar; 11. That an oleosaccharum is a more or less perfect combination, according to the oil employed.

and of water. 12. Finally, that volatile oils shaken in distilled water produce no heat, that is sensible to the thermometer, unless they have been adulterated with spirit of wine.

XIV.

XIV.

Analysis of the Schist that accompanies the Menilite near Paris: by Prof. LAMPADIUS*.

THIS mineral was formerly confounded with *polierschiefer*, or polishing slate: but Werner has given it the name of *klebschiefer*, or adhesive slate, on account of its property of adhering strongly to the tongue. On his return from France he gave me a certain quantity, that I might subject it to chemical examination. Adhesive and polishing slate formerly confounded.

The following are its external characters, as given by Werner. It adheres strongly to the tongue. Its colour is of a pale yellowish gray. It is without lustre. Its fracture is slaty, with straight leaves. It is opake. Scratching gives it a little lustre. It is very tender. It separates into leaves spontaneously, which is one of its principal characters. It is light, not being twice as heavy as water. Characters of adhesive slate.

It serves as a gangue to the menilite, with which it is found in the hill of Menil-Montant near Paris. Where found.

The following are the results of my chemical experiments on it. Analysis.

a. Roasted for two hours in a very active wind furnace it lost 30 per cent of its weight. Its colour became a deep brown. It exhibited no signs of fusion, either in a clay crucible, or in a crucible lined with charcoal: yet it had become harder, and less friable. That which had been roasted in the clay crucible was rendered very attractable by the magnet. Calcined.

b. Before the blowpipe on charcoal and with oxigen gas it melted in a few seconds into an opake glassy bead, of a blackish brown colour. Treated with the blowpipe and oxigen gas:

c. Exposed to the flame of the blowpipe simply, it was not possible to melt it: but with borax a small portion was dissolved, and coloured of a blackish brown. with borax.

These preliminary trials, and its effervescence with muriatic acid, led me to suspect, that it contained carbonic acid and iron.

* Journal de Mines, N. 106, p. 317. Extracted from a work published by prof Lampadius in 1804 under the title of *Beytrage zur Erweiterung der Chemie*.

d. A

Roasted. *d.* A thousand parts of the mineral roasted in a retort yielded 270 of carbonic acid.

Dissolved in muriatic acid. *e.* Another thousand parts dissolved in ten times their weight of muriatic acid lost 270 parts.

It contains therefore 27 per cent of carbonic acid.

I afterward proceeded with the analysis in the following manner.

Treated with sulphuric acid. 1. One part of the mineral was well powdered, and put into four parts of concentrated sulphuric acid, in which it dissolved with evident effervescence; and the solution was evaporated to dryness.

Silex. 2. The residuum was diffused in water, and a gelatinous matter separated, which was still a little yellowish. This was silex.

3. The liquor was filtered.

4. The gelatinous residuum was washed with boiling water, till no farther trace of sulphuric acid was discoverable.

5. This water and the filtered liquor were evaporated together, till there remained but ten drachms.

Lime. 6. Some sulphate of lime separated, which was decomposed by an alkaline carbonate; and after it had been heated and roasted 0·08 of pure lime were obtained.

Iron and magnesia. 7. The liquor separated from the sulphate of lime was concentrated by heat, and it yielded crystals of sulphate of iron, and of sulphate of magnesia.

8. Without separating the crystals I put the whole into a platina crucible, and exposed the saline mass to a strong heat for two hours.

9. After cooling, the mass had an ochry colour, and a bitter taste. On it I affused boiling water, filtered and washed the residuum.

Iron. 10. The oxide of iron remained on the filter. After having been dried and roasted it weighed 0·09.

Magnesia. 11. I added to the liquor carbonate of ammonia, when a white earth was precipitated, which dried and roasted appeared to be magnesia, and weighed 0·28.

12. The yellowish gelatinous residuum (No. 4) was digested in muriatic acid, till its colour became entirely white.

More iron. 13. Being filtered and washed, the liquor was of the colour

lour of pale white wine. Being precipitated with ammonia, I obtained some more oxide of iron, which washed and roasted weighed 0·03.

14. After having redissolved this oxide of iron, and that of No. 10, there yet remained 0·008 of silex. Silex to be deducted from the iron.

15. The residuum of No. 13 was found to be pure silex, which, after having been dried and roasted, weighed 0·30. Silex.

Accordingly 100 parts of this mineral contain. Component parts.

Magnesia	28
Carbonic acid	27
Silex	30·8
Oxide of iron	11·2
Lime	0·8
Water	0·3
	98·1
Loss	1·9
	100

The most remarkable circumstance is, that this mineral contains no alumine, and includes a large quantity of iron. The outward appearance of the mass would lead us to suspect the former substance, and its light colour by no means indicates so large a quantity of the second. Probably the carbonic acid combining with the oxide of iron conceals its presence*. Contains no alumine and more iron than expected.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society (14th May), Mr. P. Walker read an account of the Wernerian Society.

* Mr. Klaproth, who had before analysed a specimen of this schist, found in it: Klaproth's analysis of it.

Silex	66·5
Alumine	7
Magnesia	1·5
Lime	1·25
Oxide of iron	2·5
Water	19
	97·75
Loss	2·25
	100

birds

birds that frequent the vicinity of Edinburgh. He enumerated 178 species; of which 11 belong to the genus falco, 4 to strix, 1 to lanius, 8 to corvus, 1 oriolus, 1 cuculus, 1 picus, 1 alcedo, 1 upupa, 1 certhia, 2 sturnus, 6 turdus, 1 ampelis, 2 loxia, 7 emberiza, 8 fringilla, 1 muscicapa, 3 alauda, 15 motacilla, 4 parus, 4 hirundo, 1 caprimulgus, 2 columba, 1 phasianus, 6 tetrao, 3 ardea, 6 scolopax, 7 tringa, 4 charadrius, 1 hæmatopus, 3 rallus, 3 fulica, 4 podiceps, 4 alca, 6 colymbus, 2 sterna, 12 larus, 1 procellaria, 5 merganser, 20 anas, 4 pelecanus. This account was accompanied with interesting observations on the distinctions of several of the species, their changes of plumage at different ages and times of the year, and their kind of food; and specimens of some of the dubious species were exhibited.

Mr. Jameson, at the same meeting, read the following mineralogical queries, and stated the reasons, that induced him to consider the objects pointed out by them as deserving the particular attention of mineralogists.

Mineralogical queries.

Mineralogical queries.

1. In what species of rock are the metalliferous veins of Tyndrum situated, and what are the minerals they contain?

2. Are the leadglance veins of Strontian situated in sienite, and what are their geognostic relations?

3. Are the trap-veins, that traverse the mining field at Strontian, basalt, porphyry slate, or greenstone, or do all these different species of rock occur in that district?

4. Does the quartz-rock of Scuraben and Morven in Caithness, and of Portsoy in Bamffshire, occur in an unconformable and overlying position, or does it belong to the comformable primitive rocks, as clay slate or mica slate?

5. Does not the granular rock of Ben Nevis rather belong to the sienite than to the granite formation?

6. Does the rock of the hill of Kinnoul near Perth belong to the flœtz-trap or newest flœtz-trap formation?

7. Is the mountain of Cairnsmuir in Galloway composed of old granite?

8. What

8. What is the extent and particular geognostic relations of the black pitchstone of Eshdale Muir in Dumfries-shire?

9. Does the black pitchstone of the Cheviot hills belong to the newest flœtz-trap formation?

10. On what formation does the porphyry slate of Braid Hills near Edinburgh rest, and what are the venigenous and imbedded fossils it contains?

11. What are the geognostic characters and relations of the edge and flat coal beds or seams in Mid Lothian?

12. On what formation does the Calton Hill near Edinburgh rest?

13. Does the greenstone of Corstorphine Hill belong to the independent coal formation?

14. Does the hill on which the town of Stirling is built belong to the coal formation?

15. What are the geognostic characters and relations of the veins that traverse or are included in the greenstone of the independent coal formation?

16. What are the characters of the transition greenstone of the south of Scotland?

17. What are the particular species of petrifactions that occur in the transition limestone near the Crook, on the road from Edinburgh to Moffat?

Parkinson's organic remains.

Mr. Parkinson's second volume of organic remains of a former world is intended to be published in June. It will contain twenty plates, representing the figures of nearly two hundred different fossils of the remains of zoophytes, coloured after nature; among which are the mineralized remains of upwards of twenty species of the encrinus. It cannot but be gratifying to our readers to know, that of these remains the greater number have been found in different parts of this island.

Medical lectures.

Dr. Satterley and Dr. Young propose to give two courses of Medical Lectures next winter at the Middlesex Hospital. Dr. Satterley's will be Clinical lectures, and any of the pupils of the hospital attending them will have the privilege of seeing the patients whose cases are discussed. He will be assisted in the department of morbid anatomy by Mr. Cartwright. Dr Young's course will be on the Elements of the Medical Sciences in general, and on the Practice of Physic in particular. It has been erroneously stated in several periodical publications, that Dr. Young had a large medical work nearly ready for the press; the errour arose from his having been for some time engaged in the preparation of these lectures.

METEOROLOGICAL JOURNAL

For MAY, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

N. B. The apparatus and its relative situations will be described in our next.

MAY. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
2	57	58	62	56	29,93	Cloudy	Fair
3	60	61	60	52	—,85	Moonlight	Do. Rain at
4	61	60	76	56	—,81	Ditto	Do. [9 A.M.
5	62	60	68	56	—,83	Ditto	Do.
6	62	61	70	56	—,77	Ditto	Do.
7	64	60	69	52	—,62	Rain	Rain
8	55	53	58	48	—,57	Cloudy	Fair
9	49	48	56	45	—,54	Moonlight	Ditto
10	54	49	59	47	—,79	Ditto	Ditto
11	54	50	60	51	—,95	Ditto	Ditto
12	58	62	56	54	30,17	Cloudy	Ditto
13	61	61	67	59	—,26	Ditto	Ditto
14	68	63	74	61	—,18	Ditto	Ditto
15	70	72	77	66	—,16	Fair	Ditto
16	74	69	80	61	30,00	Ditto	Ditto
17	68	64	74	56	—, 8	Cloudy	Ditto
18	58	51	59	44	—,11	Ditto	Rain
19	54	50	57	46	—,22	Fair	Fair
20	57	55	65	54	30,00	Cloudy	Ditto
21	57	55	58	54	29,76	Ditto	Rain
22	58	57	59	54	—,50	Ditto	Ditto
23	58	54	63	49	—,66	Fair	Ditto
24	59	57	65	49	—,84	Cloudy	Fair
25	58	62	66	50	—,93	Ditto	Ditto
26	60	59	60	53	—,73	Ditto	Rain
27	59	56	61	51	29,74	Fair	Ditto
28	61	55	64	53	30,00	Ditto	Fair
29	61	60	64	56	30,18	Ditto	Ditto

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1808.

ARTICLE I.

An Essay on Polygonal Numbers, containing the Demonstration of a Proposition respecting Whole Numbers in general. In a Letter from JOHN GOUGH, *Esq.*

SIR, *Middleshaw, May* 26, 1808.

THE design of the present essay is, to demonstrate the following general propositions: every whole number is either a polygonal number of a given denomination *m*; or it may be divided into polygons of that denomination, the number of which does not exceed *m*. This singular property of numbers was discovered by Mr. de Fermat, who I believe gave it to the world without a demonstration. Should the present attempt to supply the defect obtain your approbation, the publication of it will oblige

Proposition to be demonstrated.

Yours, &c.

JOHN GOUGH.

Definitions. 1st. The sum of any arithmetical progression, beginning with unity, is called a polygonal number, and a polygon at times in the present essay.

Definitions.

2d. The difference of two adjacent terms in the generating series increased by two is called the denomination of the polygonal number; and the number is said to be digonal, trigonal, tetragonal, &c., accordingly as the denomination $m = 2, 3, 4,$ &c.

3d. The number of terms a, which are added together to form any polygonal number, is called its index; and the polygon is said to be of the first, second, or third order, &c., accordingly as $a = 1, 2, 3,$ &c.

Prop. 1. *Proposition 1st. Lemma.* Let b be the greater of two adjacent terms, $a\ b$, in a series of digonal or natural numbers; then we have $\frac{b^2-b}{2} = \frac{a^2-a}{2} + b - 1$. For $b = a + 1$, and $b - 1 = a$; but $\frac{b^2-b}{2} = \overline{b-1} \times \frac{b}{2} = a \times \frac{a+1}{2} = \frac{a^2+a}{2}$: again $\frac{a^2-a}{2} + b - 1 = \frac{a^2-a}{2} + a = \frac{a^2+a}{2} = \frac{b^2-b}{2}$, Q E D.

Corollary. *Corollary.* Hence if we put $a = 1, 2, 3,$ &c. successively, each succeeding value of $\frac{a^2-a}{2}$ may be found, by adding the next value of a in succession to that of $\frac{a^2-a}{2}$ last found, and subtracting unity from the sum; where it is evident, that when $a = 1$, $\frac{a^2-a}{2} = 0$. In this manner the annexed table is constructed, the use of which will appear in the sequel.

Table.

$a =$	1	2	3	4	5	6	7	8	9	10	11
$\frac{a^2-a}{2} =$	0	1	3	6	10	15	21	28	36	45	55

Prop. 2. *Proposition 2d.* If k be a polygonal number, of which the denomination $= m$, and index $= a$, we have $k = a + \overline{m-2} \times \overline{a^2-a}$. For the first term of the generating series $= 1$ (by *Def.* 1st); and the common difference of the terms $= m - 2$ (by *Def.* 2d); but the term of which the number

number is a, $= 1 + \overline{m-2} \times \overline{a-1}$ (Emerson's proportion, prop. 6) $= \overline{m-2} \times a + 3 - m$; moreover the sum of a terms of the series $= k$ (by *Def.* 1) $= \frac{a}{2} + \overline{m-2} \times \frac{a^2}{2} + \overline{3-m} \times \frac{a}{2}$ (proportion prop. 7); therefore $k = a + \overline{m-2} \times \overline{\frac{a^2-a}{2}}$, Q E D.

Cor. 1st. Every polygon of the first order $=1$; and every one of the second order $=m$; this is proved by substituting 1 and 2 for a in the expression $a + \overline{m-2} \times \overline{\frac{a^2-a}{2}} = k$. Cor. 1.

Cor. 2d. All polygons of higher orders may be found by the table in prop. 1st, thus: to the given index a, add the product of the corresponding number $\frac{a^2-a}{2}$, multiplied by $m-2$, and the sum is the polygon. Cor. 2.

Example—Thus the sixth decagon $= 6 + 15 \times 8 = 126$.

Prop. 3d. Let g, h, l, &c. be polygonal numbers of a common denomination m, the indices of which are b, c, d, &c. respectively; also put $b + c + d$, &c. $= a$, and let k be the polygon of which the denomination $=m$ and index $=a$; then $g + h + l$, &c. $+ \overline{m-2} \times \overline{bc + bd + cd}$, &c. $= k$. Prop. 3.
For by addition and prop. 2, $g + h + l$, &c. $= b + c + d$, &c. $+ \overline{m-2} \times \frac{\overline{b^2 + c^2 + d^2}}{2} - (\overline{m-2}) \times \frac{\overline{b + c + d}}{2}$; but $\frac{b^2 + c^2 + d^2}{2} = \frac{a^2}{2} - (bc + bd + cd)$ by involution; hence by substitution $g + h + l = a + \overline{m-2} \times \overline{\frac{a^2-a}{2}} - \overline{m-2} \times \overline{bc + bd + cd}$; therefore $g + h + l + \overline{m-2} \times \overline{bc + bd + cd} = a + \overline{m-2} \times \overline{\frac{a^2-a}{2}} = k$ (by prop. 2) Q E D.

Scholium. Put $m = 4$, and we have (by def. 2 and prop. 2) $g = b^2$, $h = c^2$, $l = d^2$; therefore $g + h + l + \overline{m-2} \times$ Scholium.

$\overline{bc+bd+cd} = b^2 + c^2 + d^2 + 2\times\overline{bc+bd+cd} = k = a^2.$

Theorem confined to squares here generalized. Thus it appears, that the present proposition generalizes a theorem formerly confined to squares, and extends it to polygons of all denominations.

Prop. 4. *Prop. 4th.* Put e and v respectively equal to $g+h+l$, &c., and $bc+bd+cd$ in prop. 3; then $v = \frac{a^2-a}{2} + \frac{a-e}{m-2}$.

For $e + \overline{m-2}\times v = k$ (by prop. 3) $= a + \overline{m-2}\times\overline{\frac{a^2-a}{2}}$ (by prop. 2); hence $v = \frac{a^2-a}{2} + \frac{a-e}{m-2}$. Q E D.

Cor. 1. *Cor. 1st.* Since b, c, d, &c., are integers, v is an integer; but $\frac{a^2-a}{2}$ is an integer, therefore $\frac{e-a}{m-2}$ is an integer; hence if $\frac{e}{m-2}$ give a quotient s and a remainder p, $a=p$ or $\frac{a}{m-2}$ gives a quotient r and a remainder p; from which we have the following general expressions $e = p + \overline{m-2}\times s$; $a = p + \overline{m-2}\times r$; $v = \frac{a^2-a}{2} + r - s$.

Cor. 2. *Cor. 2d.* Put $p = 0, 1, 2, 3 \ldots m-3$ successively, then e will be expressed as in the following table.

$p=$	0	$e=$	$0 = m-2$	$= 2m-4$	$= 3m-6$,	&c.
$p=$	1	$e=$	$1 = m-2+1$	$= 2m-4+1$	$= 3m-6+1$,	&c.
$p=$	2	$e=$	$2 = m-2+2$	$= 2m-4+2$	$= 3m-6+2$,	&c.
$p=$	3	$e=$	$3 = m-2+3$	$= 2m-4+3$	$= 3m-6+3$,	&c.

It appears from the table, that the values of e, taken vertically, constitute the series of natural numbers; therefore every integer is either a polygon of a given denomination m, or it may be resolved into polygons of that denomination.

Prop. 5. *Prop. 5th.* $b^2+c^2+d^2$, &c. $= a + 2s - 2r$. For $b^2+c^2+d^2+2v = a^2$ by involution; but $2v = a^2 - a + 2r - 2s$ (cor. 1, prop. 4); therefore $b^2+c^2+d^2 = a + 2s - 2r$. Q E D.

Cor. 1. *Cor. 1st.* $b^2 - b + c^2 - c + d^2 - d = 2s - 2r$, because $b + c + d = a$.

Cor.

Cor. 2*d.* Since a is any term of an arithmetical progression, bounded by p and e, and having $m-2$ for its common difference, prop. 4. cor. 1; it will be easily understood, that r is also a corresponding term of an arithmetical progression, of which the extremes are o and s, and common difference 1; hence it follows, that $2s-2r$ increases, while r and a diminish; therefore $b^2-b+c^2-c+d^2-d$ increases, while the sum of the roots $b+c+d$ diminishes; consequently, the number of the parts b, c, d, into which a is divided, decreases at the same time. Cor. 2.

Cor. 3*d.* If a can be so taken that $a+2s-2r=a^2$, $b=a$; and e is a polygon to the index a and denomination m. Cor. 3.

Prop. 6*th.* If e be an aggregate of polygons of any denomination m, and y that polygon, which is less than e, but greater than any polygon of an inferior order and the same denomination; then the polygons, into which e can be resolved, are equal to y or less than y. For the next superior polygon is greater than y (by prop. 2); it is therefore greater than e by hypothesis, and cannot constitute a part of it. Q E D. Prop. 6.

Cor. If $e=y+m-1$, it may be resolved into m polygons of the denomination m; namely, into y and $m-1$ units; again, if $e=y+m$ it may be resolved into polygons, the number of which is less than $m+1$; this is evident from cor. 2, prop. 4: lastly, if $e, =y+t$, can be resolved into polygons, the number of which $=m-f$, $e+f$ may be resolved into m polygons of the same denomination. It is evident, from the properties of polygonal numbers contained in this corollary, that every whole number may be resolved into polygons of the denomination m, the number of which does not exceed m. Cor.

Prop. 7. *Problem.* To resolve a given number e into polygons of a given denomination m, by help of the table in prop. 1. Prop. 7.

Method. 1st, Write down all the successive values of a, beginning with e and diminishing them progressively by $m-2$, until the series terminates with 0, or with p less than $m-2$; 2d, under each value of a place the corre-

sponding

sponding value of $s-r$, making the series begin with 0, and increase by unity, until it ends with s; 3d, these preparatory steps being completed, take any value of $s-r$ in the work, and find what numbers in the second column of the table will produce the same when added together; then if the indices of these numbers, when added in like manner, give the index of $s-r$ in the work, they are also the indices of the constituent polygons; but if the sum of the numbers taken from the table prove $=$ to the given value of $s-r$, while the sum of their indices is less than the corresponding value of a in the work, the deficiency may be made up in the latter sum by the addition of units, because one is the index of 0 in the table.

Example 1. *Example* 1*st.* Resolve 14 into pentagons. According to the directions given above, the work will stand thus:

$$e = 14;\ a = 14\,.\,11\,.\,8\,.\,5\,.\,2$$
$$s-r = 0\,.\ \ 1\,.\,2\,.\,3\,.\,4$$

Here the first value of $s-r=0$ resolves e into 14 units, because 0 in the table has 1 for its index; and $0 \times 14 = 0 = s-r$, and $1 \times 14 = 14 = a$. The second value of $s-r = 1$ resolves 14 into 9 pentagons of the first, and 1 of the second order, for 1 in the table has 2 for its index, denoting a polygon of the second order; but $2+9=11=a$ in the work. The third value of $s-r=2$ resolves 14 into 4 pentagons of the first and 2 of the second order, for 2×1 in the table $=2$, the double of the index of which $=4$ and $4+1 \times 4 = 8 = a$. The fourth value of $s-r=3$ resolves 14 into 3 pentagons, namely, into 2 of the first and 1 of the third order, for 3 in the table has 3 for its index, and $3+1 \times 2 = 5 = a$. The last value of $s-r=4$ will not resolve 14, because $3+1=4$ and the sum of their indices $=3+2=5$, which is greater than 2 or a in the work.

Example 2. *Example* 2*d.* To resolve all the numbers from 16 to 24 into tetragons or squares, which shall not in any case exceed 4 in number. It is evident from cor. 2, prop. 5, and the last example, that all the values of a may be rejected in the present instance, which are greater than the index of 16, namely 4, in the table to prop. 1, when this number

number is considered separately; consequently, the corresponding values of a in all the remaining numbers, 17, 18 ... 24 may also be rejected, and the collateral values of $s-r$ placed under the last series; which being done, the work will stand thus.

$$\begin{array}{l}
e = 16;\ a = 4 . 2 . 0 \\
e = 17;\ a = 5 . 3 . 1 \\
e = 18;\ a = 6 . 4 . 2 . 0 \\
e = 19;\ a = 7 . 5 . 3 . 1 \\
e = 20;\ a = 8 . 6 . 4 . 2 . 0 \\
e = 21;\ a = 9 . 7 . 5 . 3 . 1 \\
e = 22;\ a = 10 . 8 . 6 . 4 . 2 . 0 \\
e = 23;\ a = 11 . 9 . 7 . 5 . 3 . 1 \\
e = 24;\ a = 12 . 10 . 8 . 6 . 4 . 2 . 0 \\
r-s = \quad 6 . 7 . 8 . 9 . 10 . 11 . 12
\end{array}$$

Here the first index of $16 = 4$, and $s-r=6$; therefore $16 =$ a square of the fourth order by table; first index of $17 = 5 = 4 + 1$; $s-r = 6 = 6 + 0$; first index of $18 = 6 = 4 + 1 + 1$; $s-r = 6 + 0 + 0$: first index of $19 = 7 = 4 + 1 + 1 + 1$; $s-r = 6 + 0 + 0 + 0$; hence 19 is resolved into 4 squares, 18 into 3, and 17 into 2: again, second index of $20 = 6 = 4 + 2$, $s-r = 7 = 6 + 1$; therefore, 20 is resolved into two squares, namely, one of the second and one of the fourth order: second index of $21 = 7 = 4 + 2 + 1$, $s-r = 7 = 6 + 1 + 0$; second index of $22 = 8 = 4 + 2 + 1 + 1$, $s-r = 7 = 6 + 1 + 0 + 0$; therefore 22 is resolved into 4 squares, and 21 into 3: again, second index of $23 = 9 = 3 + 3 + 2 + 1$, $s-r = 7 = 3 + 3 + 1 + 0$; that is, 23 is resolved into 4 squares: lastly, third index of $24 = 8 = 4 + 2 + 2$; $s-r = 8 = 6 + 1 + 1$; that is, 24 is resolved into 3 squares.

In the same manner any other number may be resolved into polygons of any denomination m, so that the number of these polygons shall not exceed m, denoting the denomination.

II.

II.

On a Variety of the Brassica Napus, or Rape, which has long been cultivated upon the Continent. By Mr. JAMES DICKSON, *F. L. S. V. P. H. S**.

Great improvement of wild vegetables by culture.

IN the report drawn up by our worthy member, T. A. Knight, Esq., at the request of a Committee of this Society, and printed by their orders, it is remarked, that nature appears to have put no limits to the success of our labours in improving vegetables, if properly applied: that thus our wild crab has been converted into the golden pippin, and that our most delicious plums have originally sprung from the sloe. The vegetable which I have now the honour of laying upon your table, gentlemen, is one more among many instances of the truth of the above remark; which I have quoted, because I think it cannot be too frequently repeated, and instilled into the minds of young gardeners. Nature has undoubtedly done much in furnishing our table with a variety of esculents spontaneously, but when we aid her efforts to befriend us, by industry on our part, she, like a kind mother, never disappoints us. Who would suppose, that the hard acrid root of the brassica napus, or common rape, might be rendered so mild and palatable by cultivation, as to be preferred to the common turnip? yet this has actually been the case, and in France as well as Germany few great dinners are served up without it in one shape or other.

Synonimes.

I am unable to trace its first coming into such common use there; but, as it is distinguished by Gaspar Bauhin, who published his Pinax in 1671, it must have been well known at that period. The only synonyms I dare put down as certainly belonging to it are, brassica napus, β. *Linn. Sp. Pl, ed.* 2, p. 931; napus sativa, *C. Bauh. Pin.* p. 95; le navet, *Gallis*; Teltow rüben, *Germanis*; French turnip, *Anglis.*

Use of them.

For above twelve years I have seen this plant brought to our own market in Covent Garden, but only by one perſon: and I believe it has been chiefly sold to foreigners, though,

* Transact. of the Horticultural Society, Part I, p. 26.

when

when once known, it will be a very acceptable root in most families. It is much more delicate in flavour than our common turnip, and is to be used in the same way. In Germany, it enriches all their soups, and there is no necessity to cut away the outer skin, or rind, which is thinner than that of the common turnip, but only to scrape it. Stewed in gravy, it forms a most excellent dish, and, being white, and of the shape of a carrot, when mixed alternately with those roots upon a dish, is very ornamental. The following different receipts for dressing them are by an eminent French cook.

"Take your roots, and wash them very clean with a brush; then scrape them, cutting a thin slice away from the top, and as much from the bottom as will make them all of equal lengths: boil them in water, with a little salt, till they are tender; then put them into a stewpan, with a gill of veal gravy, two tea spoonfuls of lemon pickle, one of mushroom ketchup, a little mace, and salt, and let them just simmer, but not boil, for a quarter of an hour; thicken the gravy with flour and butter, and serve them up hot." Dish prepared from them.

"Take your roots, and after preparing and boiling them as before, put them into a stewpan, with a little water, working in as much flour and butter as will make it as thick as cream; let them simmer five minutes, then place the stewpan near the stove to keep hot: just before you dish them, add two large spoonfuls of cream, mixed with the yolk of an egg, and a little mace beat very fine, shaking the pan over the fire for two or three minutes, but do not let them boil. Put white sippets of French bread round the dish." Another.

"Take your largest roots, clean them as before, and cut them in slices as thick as a crown piece; then fry them till they are of a pale brown colour on both sides; after which, put them into a stewpan, with as much water as will cover them, to simmer for ten minutes; then add a large spoonful of Madeira or Xeres wine, the same of browning, a few blades of mace shred, two tea spoonfuls of lemon pickle; thicken the liquor with a little flour and butter, and serve them up with toasted sippets round the dish." A third.

One great advantage attending the cultivation of this vegetable is, that it requires no manure whatever; any soil that Requires no manure and is best in a poor sandy soil.

that is poor and light, especially if sandy, suits it, where it seldom exceeds the size of one's thumb or middle finger; in rich manured earth it grows much larger, but is not so sweet or good in quality. The season for sowing the principal crop is any time from the middle of July to the end of August, or even later in this country, where our frost seldom sets in before Christmas. If the season should prove dry, it will be necessary to water the beds regularly, till the plants have got three or four leaves, otherwise they will be destroyed by the fly; and this crop will supply the table till April. If wanted during the whole year, a little seed may be sown the latter end of October, and these plants, if they do not miscarry, will be fit for use in April or May. The last crop may be sown from the middle of January to the middle of February, which will also come in the end of May and June, but in July and August they will not be very good, and as at that season of the year there is an abundance of other vegetables, it is of less consequence; upon a north border, however, and in a sandy moist soil, it is possible to have them sweet and tender during the whole summer.

Mode of culture.

Saving the seed.

To save good seeds, you should, in February, or the beginning of March, transplant some of the finest roots, placing them two feet asunder, and keeping the ground repeatedly hoed: when the seedpods are formed, they should be guarded from the birds, either with nets, or shooting some, and hanging them up upon sticks. As soon as they change colour, cut the heads, and spread them to dry in the sun, after which beat out the seed, and lay it up for use.

III.

Comparative Analysis of some Varieties of Steatite, or Talc; by Mr. VAUQUELIN*.

Unctuosity of steatites supposed owing to magnesia.

IT has commonly been supposed, that the smoothness and unctuosity of the stones called steatites were owing to the presence of magnesia, because this earth had been found in every one analysed, and in consequence all the stones that had these external

* Annales de Chimie, vol. XLIX, p. 74.

external characters were classed together. But the pierre de lard, or speckstein, which in some respect may be considered as the prototype of the species, having been analysed by Klaproth, and no magnesia found in it, has changed the opinions of mineralogists on this subject, and led them to wish, that some of these substances should be analysed anew. Bildstein contains none.

With a view to remove this uncertainty, Mr. Haüy gave me three varieties of talc, that I might make a comparative analysis of them. Three varieties analysed:

The first of these is termed in Haüy's Mineralogy laminar talc. It is of a greenish white colour when seen in the mass, very smooth to the touch, and divides into exceedingly thin, flexible laminæ of a silvery white. laminar talc;

The second is called in the same work talc glaphique, because it is employed in sculpture; but commonly pierre de lard. It is the bildstein of the Germans*. This is compact, very greasy to the touch, and of a colour varying between gray, yellowish, and greenish. Its fracture is dull, uneven, and at the same time scaly. bildstein.

Of this species Mr. Haüy sent me two specimens; one of a yellowish white, from a broken Chinese image; and the other of a light rose colour, but in every other respect perfectly similar to the preceding specimen.

Analysis of the flexible laminar talc. Laminar talc

1. I calcined in a strong fire a hundred parts of this stone. By this operation it acquired a yellow colour, with a light rosy tint, was deprived of its flexibility, and lost six parts of its weight. Its laminæ being thus rendered very fragile, I could easily reduce it to powder. calcined;

2. The hundred parts thus calcined I heated with twice their weight of caustic potash. The mixture did not melt; but its tumefaction indicated, that a combination between the substances had taken place. heated with potash;

3. The mixture diluted with water was afterward dissolved in muriatic acid, and evaporated to dryness in a gentle heat. Toward the end of the operation the liquor formed a jelly. treated with muriatic acid;

4. The residuum, being lixiviated with distilled water, lixiviated;

* Agalmatolite of Klaproth, pagodite of Napione, steatite pagodite of Brongniart. Tr.

left

left a white powder, which when calcined in a red heat weighed 62 parts. It was pure silex.

precipitated with ammonia,

5. Ammonia, mixed with the liquor separated from the silex, formed in it a yellow precipitate of little bulk, from which a part and half of alumine were separated by means of caustic potash. The remainder was oxide of iron, weighing three parts and half.

and boiled with carbonate of soda.

6. After having precipitated the iron and alumine by means of ammonia, I put into the liquor a solution of carbonate of soda, and set it to boil. As soon as the mixture began to grow hot, it grew turbid and deposited a large quantity of a white powder, which when washed and calcined weighed 27 parts. This substance was magnesia, for with sulphuric acid it formed a salt, that had all the characteristics of common sulphate of magnesia.

Results of the analysis.

Flexible laminar talc therefore is compounded of

Silex	62
Magnesia	27
Oxide of iron	3·5
Alumine	1·5
Water	6
	100

Considering the smallness of the quantity of the iron and alumine, I think these substances may be presumed not to be essential to the formation of the stone; so that perfectly pure laminar talc may be deemed a compound of silex and magnesia.

Analysis of compact rose-coloured talc.

Compact rose-coloured talc.

In the analysis of this variety I pursued the same processes as in that of the preceding; I therefore need not enter into the particulars, but the following are its results.

Results of its analysis.

Silex	64
Magnesia	22
Alumine	3
Iron mixed with magnesia	5
Water	6
	100

Analysis

Analysis of the yellowish compact talc (speckstein.) Bildstein

1. A hundred parts of this stone strongly calcined lost 5 parts. calcined;

2. Heated afterward with twice its weight of potash in a silver crucible no fusion took place, but the matter was greatly increased in bulk, and had become homogeneous. heated with potash;

3. This was diffused in water, and dissolved in muriatic acid. The solution, being evaporated, became gelatinous toward the end of the operation. treated with muriatic acid;

4. The matter being dried, and washed, a white powder remained, which, after calcination, weigned 56 parts. lixiviated;

5. The silex having been separated by lixiviation, the liquor was mixed with a small quantity of muriatic acid, and ammonia was afterward poured in, which formed in it a copious white flocculent precipitate. precipitated with ammonia;

6. The liquor being filtered, the precipitate was washed and dried. This was alumine, and weighed 30 parts. The alumine dissolved entirely in sulphuric acid, and its solution, saturated with the requisite quantity of potash, afforded very pure alum: but the mother water, evaporated afresh, yielded $5\frac{1}{2}$ parts of sulphate of lime crystallized in needles. Thus with the assistance of the alumine the ammonia precipitated the lime from its solution in muriatic acid. treated with sulphuric acid;

7. The liquor from which the alumine had been separated gave no precipitate with carbonate of soda, even assisted by long boiling. The speckstein therefore contains no magnesia, like the two preceding varieties. and carbonate of soda, but nothing thrown down.

But in recapitulating the products of this analysis we find only 93 parts; namely Results of this analysis.

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
	93

A loss so considerable, which is not common in such analyses carefully executed, led me to suspect, that the compact Loss too great.

compact talc contained some other principle, which the processes employed did not enable me to discover. In consequence I treated a hundred parts, reduced to fine powder, with concentrated sulphuric acid.

Treated with sulphuric acid,

1. After boiling for two hours I dried the mixture, lixiviated the residuum with distilled water, and boiled the lixivium. At the expiration of a few days I obtained 36 parts of alum crystallized in cubes: and by a second evaporation I procured from the mother water 15 parts more of the same salt, mixed with a few needly crystals of sulphate of lime.

Cubic crystals of alum obtained.

2. As the stone appeared to me but imperfectly decomposed, I powdered it afresh, and treated it as before. On adding the acid employed in this operation to the mother water of the preceding, I obtained 15 parts more of alum, making in all 60 parts. Then, as I employed for this operation very pure sulphuric acid, and added no potash to the solution, it is evident, that the stone contained a certain portion of this alkali, and that this substance was the occasion of the loss I had in the first analysis.

Treated with fresh sulphuric acid,

and more alum produced.

Sixty parts of alum however do not require seven of potash, the quantity of loss; but as the stone is very siliceous, it is probable, that the whole of the potash was not extracted by the sulphuric acid, though I boiled the stone twice in it.

The whole of the potash probably not extracted.

The speckstein therefore is composed of

Its true component parts.

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
Potash	7
	100

Mr. Klaproth, in his analysis of speckstein, found no potash: but the quantity of water, which according to him amounts to 10 per cent, and the loss of $2\frac{1}{2}$, which he experienced, will just balance the deficiency I found. It is probable, that Mr. Klaproth estimated the water by computation,

Klaproth reckons too much water.

putation, and not by direct experiment; for, to whatever heat I exposed the stone, it never lost more than 5 per cent.

From the analyses here given it follows, that of the three varieties of talc here mentioned, two only must continue to be so called; namely the laminar talc, and the compact rose-coloured talc. The third, the speckstein, should be removed to the genus of *alkaliniferous* stones. Bildstein therefore an alkaline stone.

It is particularly remarkable, that those two varieties, which most resemble each other, and which have always been classed together, should now be separated by analysis: which shows, that minerals should never be classed according to their external appearance, since the most striking analogies in this respect are the most deceitful. Minerals should not be classed by their external characters.

In fact, the speckstein and compact rose-coloured talc have the same softness, the same fineness of particles, the same fracture, nearly the same specific gravity; and certainly, if there were any room to suppose, that one of the three substances ought to be separated from the talc species, we should be more inclined to suppose it the laminar, than either of the others.

Note. On this occasion I analysed that species of talc known by the name of *craie de Briançon*, or French chalk, and I found it to contain the same principles, and nearly in the same proportions, as the laminar talc, and the compact rose-coloured talc. These proportions were, Analysis of French chalk.

Silex	61·25
Magnesia	26·25
Water	6
Alumine	1
Oxide of iron	1
Lime	0·75
Loss	3·75
	100

IV.

Observations on the Crystallized Substances included in Lavas: by G. A. Deluc*.

Various conjectures respecting the causes and effects of volcanoes.

Have been applied too generally to geology.

VOLCANOES occupy such a striking place among terrestrial phenomena, that they have become the subject of numerous conjectures respecting their origin, their influence, and the geological consequences deducible from them. Wherever natural philosophers or geologists have imagined they might be called in to found a system, they have made them act whatever part appeared most suitable to their purpose; so that from a simple and solitary fact, single of its kind, and influencing only the ground occupied by the volcano and its vicinity; and although the volcano resembles only mountains of its own kind, and in no respect other mountains, either in shape, construction, or component parts; they have nevertheless concluded, that the strata and mountains on the surface of the Earth owe their origin to the action of fire: fire, say they, daily exhibiting to us productions identical with the primitive rocks of our globe.

Crystals in lava supposed to be formed in it.

Hence it is, that these naturalists consider the different crystals included in lava, not as products in the humid way anterior to the lava, that existed in the strata which the volcanic fires brought into a state of fusion, but as crystallizations formed in the lava itself, and from its substance, by the slow refrigeration of the mass.

This the foundation of Fl. de Bellevue's system.

On this opinion chiefly is founded the system, which Mr. Fleuriau de Bellevue has adopted, and lately published†, respecting the action of the fire of volcanoes, and the formation of the terrestrial globe, its strata, and its mountains.

Simple statement of the question.

This question, reduced to its most simple terms, is this: have the crystals included in lava been formed in the lava, and from its substance; or are they foreign to it, and formed anteriorly, in the humid way, in the strata or substances which the volcanic fire reduced to the state of fusion? And

* Journal des Mines, No. 115, p. 5.

† Journal de Physique, May, 1805.

an

an examination of this question, deduced from the true state of things, and carried by facts to a degree of evidence that excludes all doubt, would decide one of the most important points in geology, by exhibiting a just idea of volcanoes and their phenomena.

Crystals of glass chief foundation of the system.

The principal argument of Mr. Fleuriau de Bellevue is drawn from the analogy he finds between the formation of the crystals contained in lava, and that kind of crystallization, which has been called *crystallites*, and is formed in the pots in glass-houses, when the glass that was in fusion is suffered to cool slowly.

These crystals described.

Let us now examine what these crystallites of glass-houses are. The whole mass of cooled glass exhibits a confused crystallization, all of the same tint, in which we see small compact bars confusedly interlaced, some slightly striated, and others disposed in the form of stars, equally confused. At other times a number of threads are formed at the bottom of the pot, which cross and intermingle with each other, and exhibit likewise stellate figures.

In the first case these crystallites compose the substance of the glass, and are distinguished only in some places. In the second we see at the bottom, through the transparent glass, these bundles of nets, and starry figures, which have some resemblance in shape to the little icy stars, that fall with snow in very cold weather. Perhaps some instances of more decided vitreous crystallizations may occur: but these, which are rare, only prove, that there may be some circumstance favouring this crystallization in a very small space.

These said to resemble tremolite:

Mr. Fleurieu de Bellevue conceives, that these crystallite figures *singularly resemble* tremolite. This opinion, that there exists a striking resemblance between two substances, one of which is the product of a glass-house, the other of a mineral stratum, appears to me astonishing I confess; for in this way there is no substance, which we may not reckon similar to another, provided they have some similarity in form. Thus we may say, that the capillary schoerls, or mineral byssus, resemble hairs; and that the fibres of the amianthus, or stone-flax, resemble those of flax or hemp; though these substances merely resemble each other in form, without there being any real similarity between them.

but they have merely some similarity in shape.

 This

This remark was necessary, as it might be supposed, from the expression *singularly resemble*, that it was something more than in appearance, and this not very close.

Tremolite described.

The tremolite, which derives its name from the vale of Tremola near St. Gothard, one of the principal places in which it is found, is a radiated mineral substance, the threads of which, most commonly of a shining white, are united in sheaves or bundles. These bundles issue from a common centre, and diverge around it, which gives them the figure of a radiated star; and these centres being various, they give different directions to the radii, which are from half an inch in length to three inches or more. This mineral substance is one of the most curious and pleasing to the eye. It is sometimes intermixed with talc and calcareous spar; that is with two substances, one of which is vitrifiable but of difficult fusion, and the other calcinable: a circumstance of itself sufficient to exclude the least resemblance between tremolite and the products of glass-houses. And if we compare these products with the slender and brilliant threads of the tremolite, each of which taken separately has the form of a quadrilateral prism, we shall be surprised, that they were ever compared with each other. The tremolite is vitrifiable; but it is not and never was vitrified.

Cannot have been crystallized from fusion.

Crystals in lavas.

Let us now turn our attention to the crystallized substances included in lavas, to which the vitreous crystallites have been compared. This comparison I am able to make on a great number of pieces which I have collected from burning and from extinct volcanoes.

Several kinds of them, different from each other, and from the lava.

The lavas that include leucites, or white garnets, frequently include likewise volcanic schoerls, augites or pyroxenes*, and chrysolites or olivines. Here are three species of crystals, very distinct from each other both in figure and colour, contained in the same lava; enveloped in the same paste, which has no resemblance to either of them in nature, colour, or chemical properties, as will soon appear.

* I shall mention these in future by the name of pyroxene schoerls, because the denomination of pyroxene does not belong to them exclusively, all the substances contained in lavas being equally pyroxene, or strangers to fire.

The

The form of the leucites and volcanic schoerls is perfectly determinate; there is nothing in it confused, but all is precise and well marked. The leucite is constantly of a round figure, cut with twenty-four trapezoid faces, and of a gray white colour. The volcanic or pyroxene schoerl is an octaedral prism with two diedral pyramids, of a deep olive colour, and sometimes black. The chrysolite has its peridot colour, and its three crystals are found in the cellular and spongy lava, as well as in the compact.

Their figure well marked, and colour retained.

The schoerl is strongly adherent to the lava, so that it cannot be detached, and appear with its faces polished and angles entire, but by a chemical operation, the effect of the sulphurous acid fumes of the volcano. The leucite is more easily separable, leaving impressed on the lava its round form, with the shapes of its facets as clearly marked as they are on the leucite itself. Its impressions in the lava may be compared to those left by garnets, cubic martial pyrites, and several other crystallized substances, on the rocks that include them; with this difference, that the impressions of the leucite were made on a substance in fusion, and those of the garnet and pyrites on a rock that was soft from humidity.

The schoerl united to the lava.

Leucites more easily separated, leaving their impression on the lava.

Hence we may draw this inference, that the leucites were no more produced in the lava at the time of its cooling, than the garnets and pyrites were formed from the substance of the rock, which encloses them now it is dried and hardened. Both are equally foreign to the matter that contains them, and existed before it; the leucites before the lava, and the garnets and pyrites before the rock in which they are imbedded. Leucites are also found separate, and in great numbers, among volcanic ashes.

Formed before it therefore, as the crystals found in rocks.

In this exact statement of facts, can we perceive any resemblance, any analogy, between the crystallised substances included in lava, and those confused heaps of vitreous crystallites formed of the substance of the glass in the pots in glass-houses? or between those fantastic forms of cooled glass, and the crystals in the strata of our mountains, all of a constant and regular form, each in its kind?

No analogy between these and the crystals of glass.

The pyroxene schoerls too are found separate, and sometimes in multitudes innumerable. The crater that opened in

Loose pyroxenes in great numbers on Etna:

in the base of Etna in 1669, which raised a cone 4300 paces in circumference at its base, whence issued the enormous lava that we see in existence, and the bulk of which astonishes us, exhibits a singularly striking example. The summit of this crater is covered with these schoerls mixed with small scoriæ; and this remarkable circumstance attends them, the schoerls on the outside of the crater have all without exception retained on their surface a crust of the lava that included them, while those within exhibit their native polish.

those without the crater encrusted with lava; those within, not.

I will here explain the cause of this difference, and I believe I am the first who have attended to it. The sulphurous acid fumes of the volcano penetrate and decompose the surface of the lavas and scoriæ, that are exposed to it; and the schoerls, which these fumes do not attack, then appear in relief, and entire throughout, being perfectly cleaned from the lava that surrounded them; as rock crystals, when they are covered, as they are sometimes, with a calcareous tufa, are freed from it by nitric acid, and appear with all their brilliancy. This operation proves, that there is no chemical affinity between the lava and the pyroxene schoerl it includes, since the one is attacked and dissolved, and the other is not. The effect of this is sometimes a pleasing sight, as it exhibits schoerls of all sizes, even microscopic, fixed on the lava, the surface of which has been decomposed, shining with their native polish and their angles perfectly sharp.

This accounted for by the action of the volcanic fumes on the crust.

It sometimes happens, that the schoerls themselves are attacked, and their colour altered to such a degree, as to appear like small crystals of sulphur, or still whiter. This effect is produced, no doubt, when the fumes contain a mixture of acids, that act on schoerl when combined, which they could not do separately: a chemical operation of which we have a well-known instance in the aqua regia, composed of the nitric and muriatic acids.

Sometimes the pyroxenes themselves attacked.

To these facts, which evidently prove, that these crystallized substances were anterior and foreign to the lava including them, I shall add as a superabundant proof a singularity, found among the ashes that covered Pompeii, and now in my collection of volcanic substances. This is a solitary

In the volcanic ashes at Pompeii a leucite united to a schoerl.

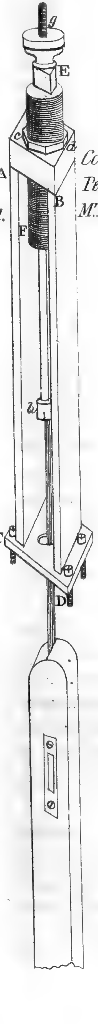


Fig. 1.

Compensation Pendulum by Mr. Henry Kater.

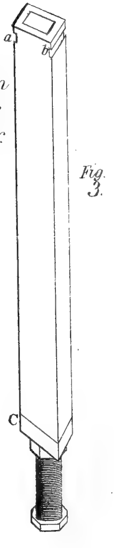


Fig. 3.

Irregular production of the Cucumber.

Fig. 5.

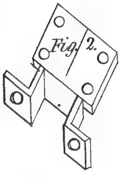


Fig. 2.

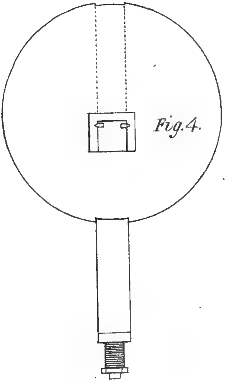


Fig. 4.

Mr. Mathew's Scapement.

Fig. 1.

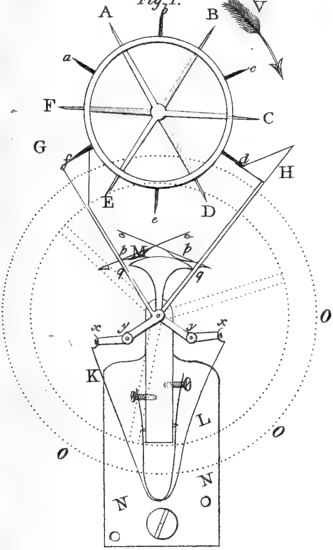


Fig. 2.

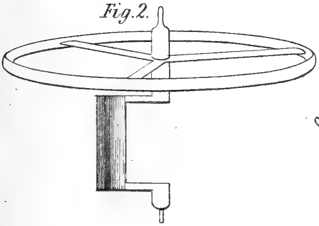
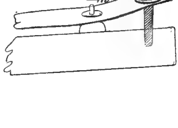


Fig. 3.



Planche's Apparatus for Succinic Acid

Fig. 4.

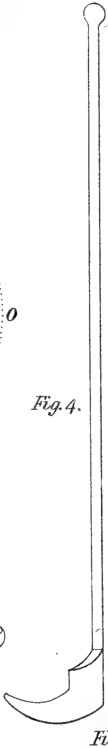


Fig. 5.

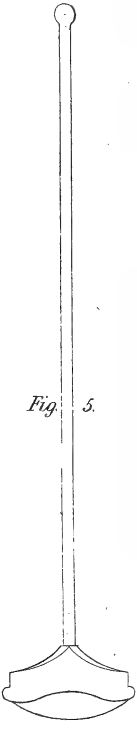
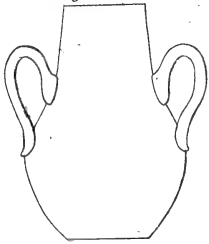


Fig. 6.



litary leucite of three or four lines diameter throughout its whole crystallization, united to a schoerl, the greater part of the prism of which it embraces. This schoerl too is perfectly crystallized, and each of these crystals retains its proper colour. It appears by some vestiges still adhering to the schoerl, that these two crystals were enveloped in a reddish spongy lava.

A leucite enclosing 3 schoerls.

This is not the only singularity I possess. I have another that came from the same place, though not so well defined, because it has retained more of the lava. This too is a leucite of the same size, perfectly distinct, and including a small groupe of schoerls, one of which is larger than the other two united with it.

Similar to rock formations.

Are not these instances similar to those that frequently occur to crystals of strata formed in the humid way? to those green schoerls, or epidotes, we see included in rock crystals; those micas, those pyrites, included in the same kind of crystal; and this in its turn enveloped in crystals of calcareous spar: unions that indicate a succession of formations. The green schoerls, micas, and pyrites, have preceded the rock crystal; and the rock crystal the calcareous spar. We find also combinations of these three crystals including each other in the same order, whence this natural consequence follows, that the pyroxene schoerl preceded the leucite in formation.

Leucites in the fissures of lavas.

I would likewise remark, that spongy lavas exhibit in their fissures leucites in part isolated, the greater part solitary, but some in groups, as happens with crystals of all kinds. Is this the course, is this the appearance, of these confused heaps of crystallites of glass cooled in the glass-houses?

No leucites in the lavas of Etna; and no crystalline laminæ in those of Vesuvius: yet both contain pyroxenes and chrysolites.

We are not acquainted with any lava of Etna that contains leucites; or any of Vesuvius that encloses those whitish crystalline laminæ, which are so abundant in the lavas of Etna. This is a fact, to which the naturalist, that supposes these crystals to be formed in the lava, ought to pay some attention. If the leucites were really formed in it, why do the lavas of Etna contain none, while they are filled with pyroxene schoerls and chrysolites, which they possess in common with the lavas of Vesuvius? Is not this difference

difference accounted for much more naturally by the absence of leucite in those strata, from which the lavas of Etna proceeded?

Lavas of Hecla

The same variations are observed in the lavas of different volcanoes. Those of Hecla, of which I have considerable specimens brought home by Sir Joseph Banks, contain neither pyroxene schoerls, nor leucites, nor chrysolites, but a great many small, white, cracked, crystalline substances, from the size of a grain of hempseed to that of a pea, of an irregular figure, having the appearance and hardness of quartz, of which they appear to be fragments. The lavas of Mont-d'Or, an ancient volcano of Auvergne, contain large crystals of amphibole, or hornblende, and feldspar, which show by their cracks and vitreous reflections, that they have experienced the action of incandescent lava; and in other ancient volcanoes in Auvergne we find pyroxene schoerls without leucites.

and Mont-d'Or.

Leucites in the lake of Andernach.

The small gravel of the volcanic lake of Andernach is filled with loose pyroxene schoerls, whole and in pieces. Should we find in this state the confused radii of cooled glass, which formed part of the mass, and could only be separated from it in shapeless fragments?

Eruption of Vesuvius in 1754.

Among the facts I adduced against the opinion of Sir James Hall, quoted by Mr. Fl. de Bellevue, whose opinion is the same, I mentioned a singular eruption of Vesuvius, that happened in 1754. A mouth opened nearly at the level of the valley, which separates the present cone from Mount Somma. At the rise of the lava this mouth formed a grotto, which it lined by its spirtings with a quantity of scoriæ in a stalactitical form, the intertwined jets of which are from three to six lines in diameter, of a reddish colour, and full of blebs. In the fragments of these jets I have found pyroxene schoerls in a state of perfect crystallization, and with their deep olive colour. These spirtings indicate, that the lava was in a high degree of fusion, and such slender jets must have cooled and hardened the moment they were separated.

Grotto lined with the stalactitical scoriæ

containing pyroxenes.

No slow cooling here.

We have here no slow refrigeration to form crystals, nor mass sufficient to give rise to crystalline forms by this mean: yet we find in these jets pyroxene schoerls, and for the

the most part even on their surface. Is not this a farther proof, that these crystals were preexistent to the lava? Mr. Fl. de Bellevue does not admit this conclusion: yet, if we attend to the fact, it will be found very convincing. The surface of the jets of this singular stalactite, and that of the interior of the blebs, are covered with a multitude of shining points, which are perceptible only by the reflection of the light. When viewed by a lens with a high magnifying power, they appear to be very slender bundles of sublimed iron.

Sublimed iron in minute particles on these jets.

I will mention another remarkable fact, the discovery of which required all the attention, with which I have examined volcanic phenomena.

The branches that separate from a stream of lava, or the lava itself when it is not abundant, break into fragments at their extremity, which in this case have no progressive motion, but by the flowing of these fragments pushed forward or to the sides by an impulse from the interior. These fragments heaped up retain their heat a long time. This is seen when they are viewed by night; and felt in the day by the heat they diffuse, as well as by the sulphurous and mephitic gasses they exhale. These fragments broken off from the lava itself, which have not ceased for a moment to be red hot, exhibit at their surface pyroxene schoerls. I possess two such fragments, which I took from the spot myself, and which have several. What can reasonably be objected to so many facts? Yet I abridge the enumeration of them.

When lava breaks into fragments, before it is cooled below redness, pyroxenes are seen in it.

" Mr. Salmon and Mr. de Buch," says Mr. Fl. de Bellevue, " have demonstrated, to all those who are acquainted with existing volcanoes, that the crystals of leucite could only have been formed during the slow refrigeration of the lava."

Opinion of Salmon and de Buch.

I am acquainted with existing volcanoes, of which I have just given proofs; yet from my observations I draw a quite opposite conclusion. The facts I have cited, which are true and exact, decide the question.

Contradicted.

With regard to the opinion of these two naturalists I will add, that it sins in an essential point. What ground is there for distinguishing the leucites from the pyroxene schoerls

Why should leucite be formed in lava more than the other crystals

schoerls and chrysolites, since these three crystals are found together in the same lava, separated from each other, and from the matter of the lava, by a line as clear and distinct as separates the small pebbles, that compose a pudding-stone, from the cement that envelopes them? If one of these crystals be foreign to the lava, so are the other two: this is a natural consequence. The fact assuredly is, that they are all three foreign to it.

Argument against their igneous formation.

The two instances I have mentioned of isolated leucites enveloping pyroxene schoerls are inexplicable facts, on the hypothesis of these crystals having been formed in the igneous way: while nothing is more common, or more easy to conceive, than such combinations between crystals of different kinds formed in the humid way.

Patrin's new Dict. of Natural History.

"I should never have done," says Mr. Fl. de Bellevue, "if I were to bring forward all the objections, that offer themselves to the system of the preexistence of crystals in lava. Several will be found at the articles Lavas and Leucites, in the new Dictionary of Natural History, in which Mr. Patrin has strongly combated these suppositions."

We should regard facts, not suppositions.

I am sorry to learn this, since the readers of that Dictionary, who are desirous of knowing what lavas and leucites are, will be led into errour. I have exhibited *facts*, and not *suppositions*. In the physical phenomena of our globe, the accurate knowledge of which depends always on matters of fact, I never was fond of suppositions, which seldom fail to lead us into some mistake.

In the lava of Viterbo leucites calcined.

Let me remind Mr. Fleuriau de Bellevue of a very remarkable lava of the ancient volcanic mountain of Viterbo. This lava contains a multitude of leucites from the size of a large pea to that of a rapeseed. These leucites have undergone a kind of calcination, which renders them very white, and the lava that includes them is black, which occasions a striking contrast between these substances. Nothing more strongly marked can be conceived. Now is it not evident, that all these leucites existed before the lava? If we dispute this conclusion, we may as well deny, that any foreign substance whatever, included in a rock, has existed before the rock.

Leucites acted

Leucite does not resist the action of volcanic fires and vapours

vapours in the same degree as schoerl; as their effects upon it appear to be not inferior to those upon the lava. At least none of the pieces I possess, that have been exposed to their action, afford any leucite in good preservation; though it retains its characteristic form in the midst of red hot lava. When the heat is carried higher, it is capable of softening it, and occasioning it to undergo a sort of calcination. It then cracks, and the matter of the lava penetrates these cracks in the leucite; whence we perceive within it particles of lava, which are distinguishable by their black or brown colour, and little blebs: but the form of the leucite is preserved, because, as the lava entirely surrounds it, no part of its surface can separate from it. This is the case with the leucites of the ancient lava of Viterbo; and on the piece in my possession there are several niches of leucites, with the impression of their facets. The lava and leucites coming together out of the fires of the volcano, as the lava there must be more perfectly fused than when it flows exposed to the open air, and in its subterranean course must meet with narrow passages in which it is compressed, its matter must penetrate more easily into the cracks of the leucites.

upon by volcanic fumes more easily than schoerl.

It has been said, that no leucites are found in lavas that have flowed with rapidity, but that they are confined to such as have flowed slowly. This is a mere ideal distinction: for by what signs can we determine, whether a lava have flowed slowly or rapidly? I fancy it would puzzle any man, to determine this with certainty: and besides, what change can the less or greater velocity or slowness of its course occasion in the substance of a lava?

Not easy to determine whether a lava have flowed slowly.

The following is a very remarkable fact related by Mr. Dolomieu. "Loose leucites are so abundant in the vicinity of Rome, that the road from Rome to Frescati may be said to be covered with them. The rain washes them away, and collects them in vast quantities in the ditches by the roadside." To this fact Mr. Dolomieu subjoins some conjectures respecting the origin and formation of the leucites, in which I think he is mistaken, though he is far from supposing them to have been formed of the matter of lavas.

Loose leucites abound near Rome.

I have not seen this singular place, but I possess a pretty large

They come

from decomposed lava.

large number of these very leucites, from the smallest size to that of a little cherry. They must have come from spongy lava at no great distance, that has been decomposed. I have seen some of the same nature near Civita Castellana; the whole surface of which was spotted with a multitude of white grains. Unfortunately, and to my great regret, it rained very hard at the time, which prevented my alighting from my carriage. How can we conceive, that the multitude of loose leucites at Frescati were formed from the substance of the lava that included them? They are a little transparent, and of a slightly yellow colour: is there in this any analogy with the colour or matter of lava? Indeed we might as well maintain, that the garnets included in a rock have been formed of the substance of that rock.

Similar ones near Civita Castellana.

Crystals said to be formed in the crater.

Mr. Fl. de Bellevue imagines, that the crystals thrown out separately by the crater "are new products, formed in the crater itself by a first cooling."

None but by sublimation.

Nothing is formed in the crater, or to speak more accurately on its sides, but crystals of salts and sulphur by sublimation; and never any crystal of solid matter like those contained in lava.

The lava said to cool in the crater, before its eruption.

To support this opinion he fixes two epochs: the first of which, according to him, takes place in the crater itself. A first cooling in the crater! But let us admit this supposition. Thus we have a lava cooled and hardened: but from a lava come to this state none of the substances contained in it could be separated so as to appear loose: for this it must be plunged again into the fire of the volcano; and would it not there enter again into fusion?

The crystals merely thrown out by the explosions.

Crater on Etna of 1669.

The crystals that are found detached on the cone of a crater have been separated in the crucible of the volcano itself by the ebullition of the melted lava, and the jets of its explosions. The crater that opened on Etna in 1669 exhibits a very instructive example. The very large cone raised by this opening is covered with an innumerable multitude of pyroxene schoerls, all without exception covered by a slight crust of the lava that contained them, mixed among the small scoriæ in which some are included. This lava cannot have been cooled for a moment from the first instant

instant of its fusion; yet we here find a multitude of crystals, that issued from the crater ready formed. Can these have been produced by a first cooling of the lava? The enormous mass of this lava, that issued from the foot of the cone, contains itself a prodigious quantity of these schoerls: all their edges are distinguishable on the surface of the fractures.

Another fact respecting this lava.

This same lava, and the jets of its explosions, exhibit another interesting fact. It includes, beside the pyroxene schoerls, a multitude of small crystalline laminæ of a whitish colour, that have no regular form, and appear to be scales from some substance splintered by the heat. These laminæ are found detached likewise, mingled with the schoerls and little scoriæ. Can we discover here that *play of affinities*, to which the formation of the crystals included in lava is ascribed, since here is no regular form? Besides, the play of affinities can take place only when the molecules, on which they act, are at liberty to unite, which they cannot be, except in a mass perfectly fluid: and this is not the state of lava, in which it is asserted to occur. They are in fusion, no doubt; but it is a dull, heavy fusion, that has no progressive motion but on steep descents, or from the successive impulses given by the matter that issues from the volcano, and pushes before it, while at the same time it spreads at the sides, that which preceded it. How can affinities be exerted in such a mass?

Cannot arise from the action of affinity.

Matters thrown up by the explosions contain pyroxenes

The burning matters thrown up by the explosions of the crater, some of which are drops of compact lava, others fragments torn from the mass in fusion, contain likewise pyroxene schoerls, which show themselves entire, when these fragments have been exposed to the corrosive action of the vapours of the crater. This action is sometimes carried so far, as to reduce these fragments to a degree of softness little less than that of dough: and the schoerls there being in perfect preservation, they are well distinguished by their black colour on the yellow sulphurous tint of this paste, which acquires some consistency in drying, but is easily broken. I have collected several pieces in these different states, which are now before me. We cannot suppose, that there was a moment of first cooling in this case: since

since these pieces were thrown from the focus of the volcano, at the very moment when its contents were in the highest fusion.

The products of volcanoes should be compared with those of art.

"One of the most natural ideas, that present themselves, to solve so many difficulties," says Mr. Fl. de Bellevue at the outset, "must be carefully to compare the products of volcanoes, and the circumstances in which they are found, with the results of those large bodies of fire, by means of which man separates, dissolves, brings together, and combines minerals, and produces in them a change of form." This I have just done. I have compared the products of our glass-house furnaces with those included in lava, and the result of my comparison is, that they are totally different.

This done.

(To be concluded in our next.)

V.

Experiments on Molybdena: by CHRISTIAN FREDERIC BUCHOLZ.

(Continued from p. 138.*)*

VI. *Phenomena presented by molybdena exposed to the action of fire in contact with atmospheric air.*

Molybdena calcined.

Exp. 22. A Piece of molybdena in the metallic state, weighing fifty-three grains, of a moderate consistency, and an ashen gray colour, was put into a Hessian crucible, and the heat raised gradually. Scarcely had the heat reached a deep red, when the surface of the metal became of a brownish yellow, and soon changed to a fine violet, inclining to indigo. The metal being withdrawn from the fire and broken, its central part was still gray, and had undergone no alteration. From this nucleus to the surface the colour proceeded in gradation through a yellow and brownish yellow to blue. The metal having been again exposed to the same degree of fire for a sufficient time, it became entirely blue;

Oxided in different degrees.

blue: but many precautions were necessary to attain this result, because the surface passed very readily to a higher degree of oxidation, and quickly reddened. On this blue mass I poured cold water, which partly dissolved it; and by boiling I completed the solution, which was equally of a blue colour.

Blue oxide.

Dissolved in water.

When the crucible, in heating it more strongly, passed to a deep red, the metal quickly began to burn, putting on likewise a deep red appearance. At this degree of heat it kept its deep blue colour. The fire being increased, the metal was brought nearly to a white heat, and after cooling, its surface, to the depth of a few lines, was of a blueish white; nearer the centre it was of a blue inclining to violet; and the nucleus was violet inclining to brown, like the matter obtained in decomposing molybdate of ammonia by heat. The metallic mass, which had little consistency till the action of the fire had given its surface a white colour, became more compact and tenacious, so that it was difficult to crumble it between the fingers. On urging the fire the whole surface became enveloped by the molybdic acid that was formed; and this acid gradually increased in quantity, till at length it entered into fusion.

Exposed to a greater heat.

Acid formed.

These phenomena evidently indicate different degrees of oxidation. The brownish oxide may be considered as the first degree. The violet brown oxide is very probably at the same degree of oxidation, as that obtained by exposing the molybdate of ammonia to a red heat. The blue oxide soluble in water seems to contain a larger quantity of oxigen; while the blueish white oxide may be considered as a mixture of the blue oxide with white oxide, the last of which is probably nothing but molybdic acid, that fuses and sublimes at a higher heat. Thus these different oxides may be arranged in the following order: the light brown, the violet brown or violet, the blue, and the white.

Different degrees of oxidation.

Their order.

Of these oxides the blue chiefly attracted my attention, more particularly on account of the different manners, in which it may be produced by oxidation and disoxidation, in the treatment of molybdena by acids, alkaline sulphurets, metallic solutions, &c.

Blue chiefly noticed.

Exp.

Experiments on the blue oxide of molybdena.

Experiments on this.

Exp. 23. Fifty grains of metallic molybdena powdered were put into a porcelain crucible, placed in a sloping direction on the fire, and heated till the surface acquired a blue colour. On first heating the powder became of a brownish yellow, which soon changed to a copper brown. This colour remained some minutes, till the crucible acquired a greater heat. The metal burned in a part where the crucible scarcely began to be of a dull red. Immediately on this I drew back the crucible, and kept it for a quarter of an hour exposed to a moderate heat, constantly stirring the powder. The brown colour thus changed completely to a grayish blue, and the powder carefully collected and weighed had gained an addition of five grains, or one tenth. Having poured on it an ounce of water, and shaken it a few minutes, a very small portion only was dissolved. On keeping the mixture for two hours at a heat of 30° [86° F.] the solution assumed a deep sapphire blue colour, and a bitter metallic taste. Having decanted the solution, and poured a fresh quantity of water on the residuum, I proceeded as before, and obtained a very pale blue solution. The residuum I boiled with two ounces of distilled water in a china cup till half the fluid was wasted; and when the powder had subsided, I had a fine deep sapphire blue solution. The same thing took place on repeating this process.

Not so soluble as in the preceding experiment.

Thus the oxide formed in this experiment did not comport itself like that obtained in the preceding (*Exp.* 22.) where the blue oxide obtained by the calcination of metallic molybdena dissolved in water completely. In this present case the blue oxide appears to have penetrated the rest of the mass, and prevented the whole from being oxided to this point, by which it had become itself less soluble.

Residuum.

The residuum when dried weighed twenty grains, and was of a dark gray inclining to brown, which led me to believe it was a mixture of brown oxide and metal. I then put it

Heated again.

again into the cup, and roasted it cautiously; and in fact, as soon as I began to heat it, its colour changed to brown inclining to blue, till by degrees it became entirely blue. After having boiled it three different times with two ounces of

of water till half was evaporated, I obtained a blue solution. Still I had a residuum of fifteen grains, which was of a copper brown inclining to blue. This I set aside for the present, and made a trial with a large quantity of metal, in order to find a readier method of obtaining the blue oxide.

Still a residuum.

Exp. 24. I reduced two hundred grains of metallic molybdena to as fine a powder as possible, and treated this as in the preceding experiment. A copper brown oxide was formed, which became blue on continuing the heat. When it was nearly of an indigo blue, with a tint of gray, and began to burn in different places, I withdrew it from the fire, put it into two ounces of water, and boiled it till half was evaporated. A blue solution was thus obtained, and the residuum was treated three times in the same way. The last residuum had entirely lost its blueness, and acquired a copper colour: however, I boiled it thrice more, and the solution was still blue. This is an evident proof, that simple boiling in water changes the brown oxide into blue oxide, and consequently that the latter is more oxided.

Exp. 24.

Boiling changes the brown oxide to blue.

I now attempted actually to convert the brown oxide that remained into blue oxide by continued ebullition in water, and for this purpose I put it into a large vessel with sixteen ounces of distilled water, which I boiled till it was reduced to two. The solution was blue it is true, but not to such a degree as I expected from so long boiling. I therefore tried whether the brown residuum would not be more easily changed into blue oxide, if I merely moistened it and afterward dried it repeatedly. This I did ten times; and each time I poured an ounce of water on the residuum, which I boiled for five minutes. The solution was still blue, and in this way I reduced the brown oxide to eleven grains.

Attempts to effect this completely.

This mode of preparing the blue oxide is very troublesome, I was sensible of the defect, and I sought by several methods to find a better. I had observed, that, when a solution of molybdena in sulphuric acid is decomposed by an alkaline sulphuret, and that afterward a little sulphuric acid is added, the precipitate, that was formed in the first instance, is decomposed, and a blue solution is produced. But

Trials to produce the blue oxide with less trouble.

With sulphuric acid.

Could not be collected on account of its solubility.

But I could discover no method of collecting the blue oxide in its pure state; for, after I had evaporated the solution of this oxide, I could not separate the residuum, on account of its great solubility, either from the sulphuric acid, or from the alkaline sulphate formed by means of the sulphuretted alkali. A portion of sulphur too remained in this residuum. It is true the alkalis separated a small quantity of oxide, when the solution was concentrated, but its solubility did not permit me to wash what was on the filter. I must observe too, that an excess of alkali destroyed the blue colour; consequently it is probable, that it occasioned a higher degree of oxidation.

Blue colour destroyed by an alkali.

Other acids turn the molybdic blue.

To effect the separation sought, I endeavoured to avail myself of the experiment of Scheele and other chemists, namely, that molybdic acid, when dissolved in other acids, affords a blue liquor. The muriatic acid appeared to me most proper, on account of its volatility. Accordingly I dissolved two drachms of brown oxide, obtained by calcining molybdate of ammonia, in moderately concentrated muriatic acid. The solution during ebullition changed from brownish yellow to yellowish green, and lastly to a deep blue. I evaporated to dryness, and obtained a mass of a dull blue, but I could not free it entirely from the acid that adhered to it. On washing it, it was partly dissolved, and what passed through the filter, as well as what remained on it, contained muriatic acid. If I heated the blue mass more strongly, it became gray, and was deprived of its solubility in water as well as of its muriatic acid. After several unsuccessful trials varied in different ways, I was at length led to the object I sought by the consideration of a simple fact, namely, that a solution of molybdic acid assumes a blue colour by the contact of most metals. I conceived it would be the same with molybdena itself, and that this metal, participating in the oxigen of the molybdic acid, would change it to the state of blue oxide.

Muriatic acid.

Could not be entirely separated.

Most metals change the molybdic acid blue.

12 grs. of molybdena and 24 of its acid in water form blue oxide.

Exp. 25. In consequence I took twelve grains of metallic molybdena and twenty-four of molybdic acid, reduced the whole to a very fine powder, and put this into seven ounces of water. After standing ten minutes the liquor assumed a blue colour, which grew deeper and deeper. On boiling

boiling for half an hour the solution was found to be much stronger than in any of the preceding trials; and on boiling it a second time the whole of the acid and metal had disappeared, except two or three grains, being converted into blue oxide.

I was now desirous of trying, whether I could not obtain the blue oxide in a still simpler and cheaper manner, by employing the brown oxide obtained from the decomposition of molybdate of ammonia instead of metallic molybdena.

Exp. 26. A hundred grains of molybdic acid and eighty of brown oxide were triturated together, and reduced to a very fine powder. This powder being wetted, after some time a blue colour appeared, but much more tardily than when metallic molybdena was employed. After triturating this mass however for a quarter of an hour, the magma was very blue. It was then boiled four times, with four ounces of water each time, and the whole was dissolved except a few grains. The solutions were blue.

100 grs. of acid and 80 of brown oxide did the same, but more slowly.

Several other trials convinced me, that molybdena in the metallic state acts more powerfully than the brown oxide in converting molybdic acid into blue oxide. I found too, that by long triturating a mixture of metallic molybdena and brown oxide, and adding water from time to time, so as to keep the mixture of the consistence of pap, the greater part of the mass might be converted into blue oxide.

Molybdena converts the brown oxide into blue by trituration in water.

When the mixture was dry, I poured extremely pure water on it, when a smell nearly resembling that of oil of rosemary, faintly inclining to that of camphor, was very sensibly emitted. This is a very extraordinary circumstance; but if any one should doubt the fact, I can appeal to the testimony of Messrs. Trommsdorff and Haberlé, who were with me when I made the experiment. Perhaps the cause of this might be discovered by operating with a larger quantity of materials.

Extraordinary smell from it.

Exp. 27. I took all the solutions of blue oxide in pure water produced in the preceding experiments, poured them into a porcelain capsule, and boiled them down to the consistence of a sirup. As the liquor boiled it grew lighter coloured, till at length it appeared of a deep steel gray; and

The blue solutions evaporated.

and after it was cold it resembled altogether a concentrated solution of acetite of copper inclining a little to blue, in other words, it was of a deep blueish green; it was of a metallic and bitter taste; and no precipitate was formed. The addition of a little muriatic acid appeared to restore the original blue colour. This experiment evidently shows, that the blue oxide is capable of passing to a higher degree of oxidation by the effect of simple boiling in water; and that this degree of heat must if possible be avoided, when we wish to obtain blue oxide. Several other experiments, which it would be superfluous to detail here, taught me, that the following process is best adapted to produce a pure and permanent blue oxide.

Best process for obtaining pure and permanent blue oxide.

Take one part of metallic molybdena and two parts of pure molybdic acid (or three parts of brown oxide and four of acid), triturate them a considerable time in a porcelain or glass mortar, moistening the mixture with distilled water, either at the beginning or after it is reduced to a fine powder, so as to give it the consistency of pap. Continue the trituration with a moderate heat, till the matter is very blue. Then add eight or ten parts of water, and boil for a few minutes. After the liquor has stood a little while, filter it, and continue to triturate and lixiviate the residuum, till no more blue solution is obtained. All the solutions being poured into a porcelain capsule, they are to be evaporated at a heat of 30° or 40° of R. [100° or 122° F.], when the colour will undergo no sensible change, and a very fine blue residuum will be obtained, which is soluble in a very small quantity of water.

Evaporation must not be too slow.

Care must be taken, that the evaporation does not go on too slowly; for I think I have observed, that in consequence of being in contact with the oxigen of the atmosphere, the blue oxide passed gradually to green, yellow, and lastly even to white molybdic acid. At least I have remarked these phenomena, when potash or ammonia was present in the blue solution.

Means of preventing too high oxidation.

This accident however may be prevented effectually, by leaving a little metallic molybdena or brown oxide in contact with the solution evaporated, till the liquor has attained the consistence of a sirup. This will prevent the oxigen present from producing a higher degree of oxidation.

From

From the experiments on the blue oxide, that have been related, we may dedúce the following results. General results.

1. Several of the degrees of oxidation before observed have been confirmed, and some others discovered. In the experiments made on metallic molybdena I have frequently remarked, that its surface lost its splendour, and seemed to become coated with a *gray* matter.

This is certainly a commencement of oxidation, and is the first stage: the *brown* oxide is the next: and this passes, as has been said, by boiling to the *blue;* which may likewise be produced by heating the metal, or by heating the brown oxide obtained by the decomposition of the molybdate of ammonia; and it appears, that the substance produced by these two different operations may be considered as identical. After the blue oxide we have the *blueish green,* which may be produced by boiling the blue, or leaving it exposed some time to the air: and the contact of metallic molybdena, or the action of pure ammonia, will bring this back again to the state of blue oxide. Lastly the blueish green oxide passes to *yellow,* and afterward to *white,* which is the molybdic acid. The transmutation of the blue oxide into the last two is singularly promoted by the presence of an alkali. Different stages of oxidation. Alkali promotes a higher oxidation.

2. The white molybdic acid placed in contact with the brown oxide, or with metallic molybdena, divides its oxigen with them, and thus passes to the state of blue oxide. The blue colour, that molybdic acid acquires on the addition of a metallic solution, as remarked by Scheele, Heyer, and Ilsemann, is an effect of a similar disoxigenation. Other disoxigenizing circumstances may occasion the conversion of the molybdic acid to the state of blue oxide, as for instance the passing of ammoniacal gas over it. Molybdic acid converted to blue oxide.

After having discovered these different degrees of oxidation, it appears an object to ascertain the proportion of oxigen to the metal in each. This I shall pursue with some other inquiries, when I have procured a sufficient quantity of the ore of molybdena. The principal subjects of my research will be the blue and the brown oxide, as they are the most stable, and are most easy to procure in large quantity and unmixed: but what renders them particularly interesting Proportions of oxigen a subject of farther inquiry.

teresting is, that they frequently occur in various operations on molybdena, I shall confine myself here to a few of the principal properties of the blue oxide. 1. It comports itself altogether as an acid. It reddens blue paper more quickly and more powerfully than the white acid; and it produces a brisk effervescence on combining with alkaline carbonates, with which it furnishes a blue solution. We see here a base combined with a certain quantity of oxigen manifesting a stronger acidity, than when it contains a greater quantity of the acidifying principle; a very remarkable anomaly. 2. This acidity still remains when the blue oxide has passed to the state of blueish green oxide (which reverts to its former state on the addition of an alkaline corbonate). Its preparation shows its solubility in water, but I have not yet ascertained the quantity water will take up.

Blue oxide acts as an acid more powerfully than the acid itself.

Blueish green oxide also acid.

Exp. 28. The manner in which metallic molybdena comports itself, when heated in the open air, has already been seen. Some phenomena, that occurred when I was ascertaining its specific gravity, led me to examine what would take place on leaving it in contact with water at the ordinary temperature. For this purpose I took thirty grains of powdered molybdena, put it into a porcelain capsule, and wetted it with water, which I left to evaporate slowly. Having poured fresh water upon it, this afterward acquired a blue colour: and this process being several times repeated, the whole of the metal was converted into blue oxide. The different degrees of intermediate oxidation observed in my other experiments did not occur here. The brown oxide, treated in the same manner, produced a similar result.

Molybdena converted into blue oxide by water at the common temperature.

Brown oxide the same.

(To be concluded in our next.)

VI.

Remarks on the Formation of Acetous Acid in Cases of Indigestion; by Mr. Perperes, *Apothecary, at Azilles. Communicated by Mr.* Parmentier*.

Acetic acid supposed to be formed in the stomach

CHEMISTS at present are agreed in opinion, that acetic acid is formed during the digestion of certain substances;

* Annales de Chimie, vol. LX, p. 280.

stances; the experiments here given therefore only confirm what has already been advanced. They do not equally admit however, that the spirituous fermentation may take place there. This our author asserts; but as it is a mere assertion, the subject deserves farther inquiry.

Physiologists have long been convinced, that fermentation is necessary to digestion, and that it may be of the spirituous, acid, or putrid kind, Some alimentary substances are liable to all three, as several observations show: but in general every article of food undergoes that fermentation, which is most analogous to its nature. Vinous, acid, and putrid fermentations take place in it.

As we are acquainted with one kind of ſpirituous fermentation only, and one of putrid, I shall make no mention of these two fermentations. But the same cannot be said of the acid fermentation, since it gives rise to several acids differing in their nature, with which it is of importance to become acquainted, to fix the opinion of chemists on this head. Accordingly I determined to make on myself the experiments I am about to describe. Experiment to ascertain the nature of the acid formed in indigestion.

1. Knowing that my stomach is incapable of digesting roasted chesnuts without great difficulty, occasioning always eructations, followed in a few instants by insupportable heartburn; I took eight ounces, which I ate without bread, after fasting thirteen hours, and having my stomach perfectly empty. Eight ounces of roasted chesnuts eaten. An hour and half after eating them I experienced a swelling in my stomach, an effect that amylaceous substances commonly have with me. Their effects. This indicating the production of some gas, I prepared to collect it in the following manner. I took a funnel with a long semicircular pipe, the small end of which opened under a jar filled with water in the pneumatic trough: and I took care to fill the trough with water, so as to cover the whole of the tube, that I might lose none of the gas, which I knew I should emit. In fact, a few minutes after I felt my stomach still more dilated, and soon had eructations, which I collected by putting my mouth into the funnel. Thus I obtained at different times a cubic inch of gas, which had all the characters of carbonic acid, and a small portion of atmospheric air, which the food always carries down with it, as being necessary to digestion. Carbonic acid gas produced.

The

Heartburn.

The swelling of my stomach being diminished by the abstraction of this carbonic acid gas and atmospheric air; and the acid fermentation appearing from the heartburn I began to feel, to have gone through all its stages; I prepared to follow up my experiment.

The contents of the stomach examined.

2. The object was to ascertain the nature of the acid contained in my stomach: and this could only be done by bringing it up, that I might find out its specific characters by subsequent examination.

Accordingly I took twenty grains of ipecacuanha, mixed with three ounces of distilled water, at one dose: and a quarter of an hour after I drank warm distilled water to the quantity of fourteen ounces, without producing any effect. Three ounces more however made me bring up at twice all I had taken.

Part perhaps had been absorbed.

On weighing the whole, I found it amounted to two ounces less than I had eaten and drank. Whether the stomach had digested the two ounces of fluid that were deficient, or they had been absorbed, I cannot say.

Appearance.

What I had brought up resembled feculæ diffused in water; which showed, that the fermentation had destroyed the alimentary substance I had eaten, particularly as the smell was strongly acetous. This smell confirmed the idea I had long conceived, that vinegar is formed in cases of indigestion, and encouraged me to go through with my experiments.

An acid present.

3. I dipped litmus paper into it, which was immediately reddened. A little dropped into an infusion of violets reddened it instantly. Thus I was convinced of the existence of an acid, and I had recourse to the following means for ascertaining its nature.

Distilled.

4. Having put the whole into a glass retort, I adapted to it a globular receiver, furnished with a tube of safety, and with a curved tube terminating under a jar in the pneumatic apparatus, to receive the gas that might be dissolved in the matter I was examining. The whole being luted, I heated it gradually till it boiled; and kept it in this state till the matter in the retort began to acquire consistency. I then unluted my apparatus, and found in the receiver sixteen ounces and half of a very clear fluid, the smell

Products vinegar,

smell and taste of which resembled those of distilled vinegar, and which had all the properties of an acid. The only aeriform product was a little carbonic acid, which was very distinguishable by the rapidity with which the bubbles rose through the water, when it was once saturated, as well as by their magnitude.

and carbonic acid.

5. Though the smell and taste of the product of the distillation already gave me strong proofs of the existence of acetous acid, I thought it necessary, to have farther confirmation. I therefore took some soda purified by means of alcohol, and added it to the whole of the liquor to supersaturation. I then filtered, and evaporated to a proper degree for obtaining crystals of acetate of soda. I put the porcelain vessel, that contained the saline liquor, in a cool place; and the next day, to my great satisfaction, I found, that the form of the crystals, which were striated prisms much resembling very small crystals of sulphate of soda, was precisely that of acetate of soda. Their taste too was bitter, pungent, and giving an acrid taste at the beginning, which afterward finished with being alkaline; in short they were in every respect similar to acetate of soda prepared directly from its component parts.

The acid saturated with soda.

6. Still fearing, that these experiments might not be sufficiently demonstrative, I was desirous of satisfying myself still farther. I therefore took half an ounce of the saline substance I had obtained, and dissolved it in six ounces of distilled water. This solution I divided into two equal portions: into one I poured gradually very fine sulphuric acid, but not sufficient to decompose it entirely; and into the other my solutions of barytes. The first portion, which had been decomposed by sulphuric acid from its greater affinity with soda, was put into a small retort, to which I adopted a receiver, and distilled with a gentle heat. The product in the receiver was acetous acid, perfectly pure, and with a very fragrant smell, in fact having all the characters of that acid. Into the second I poured a solution of barytes, till the soda was set free: and into the phial containing the solution of soda and acetate of barytes I poured a sufficient quantity of alcohol to dissolve the soda, and precipitate the acetate of barytes. Thus I was completely

The salt dissolved,

divided into two portions,

one decomposed by sulphuric acid and distilled,

the other by barytes.

pletely satisfied, that it was acetous acid; and all my suspicions were realized.

Conclusions.

From the experiments I have related it follows:

The distension of the stomach owing to carbonic acid gas:

1. That the distension experienced by the stomach in cases of indigestion is occasioned by the formation of carbonic acid, arising from a commencement of decomposition, which the nutritive substances taken as food, chiefly when they are of the amylaceous vegetable kind, have undergone.

and the heart-burn to acetous acid.

2. That the burning pain which the digestive organ experiences, and which sometimes extends to the œsophagus, is owing to a quantity of acetous acid, that is formed by the complete disoxigenization of the aliment.

8 oz. of chesnuts produced 2½ oz. of acetous acid.

3. That eight ounces of roasted chesnuts produced two ounces and six drachms of acetous acid, after having fermented in the stomach an hour and half.

Remedy.

4. Finally, that the method of remedying this disagreeable sensation, which frequently occurs to persons who have weak stomachs, is to take after a meal ten grains of powdered colombo-root, with twelve grains of calcined magnesia, mixed together for a single dose. This remedy has constantly succeeded with me.

VII.

On the Cause of Animal Heat. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Query respecting animal heat.

DR. Reade in his observations in your last Number, on the generation of heat in water by agitation, offers a query; "Whether friction be adequate to account for animal heat."

Friction not adequate to its production.

If the Dr. mean by friction, that which is produced by the impetus or flow of blood in the arteries, it would appear very improbable, that the friction thus produced should be capable of supporting animal heat: on the contrary, I have been induced to believe, from analogous experiments, that animal heat is produced and supported by *gradual combustion*, produced by the action of the oxigen of the atmospheric air, which is inhaled by the lungs, on the iron of the blood,

Owing to combustion of iron in the blood.

blood. At each inspiration a new action is produced: and the attraction of oxigen for iron needs no observation from

Your very obedient humble servant,

Bristol, June 16th, 1806. P.

ANNOTATION.

THE supposition, that animal heat is kept up by a process analogous to combustion is by no means new: but it is far from probable, that it should be by the combustion of iron. If we consider the quantity of heat necessary to preserve the warmth of so large a mass as the human body under certain circumstances, and requiring constant renovation, where shall we find a sufficient supply of iron, to generate by its combustion the heat actually produced in many cases? or what becomes of this iron afterward? Though iron appears to be pretty generally diffused, and to enter in small portions into perhaps most animal and vegetable substances: yet the ingesta, particularly in fevers, where much heat is produced, and little nourishment taken, can scarcely afford enough to account for the heat, or for the oxigen consumed in the air vitiated by respiration. And on the other hand the proportion of oxide of iron in the blood will scarcely be found answerable to the supposed effect. Facts however are always valuable; and a series of well conducted experiments, whatever they may tend to prove, or whatever hypotheses they may contradict, cannot fail to be interesting to the impartial inquirer. This not probable.

VIII.

Description of a Species of Ox, named Gayál. Communicated by H. T. Colebrook, *Esq**.

THE *gayál* was mentioned in an early volume of the researches of the *Asiatic* Society†, by its *Indian* name, which was Gayál has been mentioned but not described.

* Abridged from the Asiatic Researches, vol. VIII.

† In the second volume, (p. 188,) published in 1790.

was explained by the phrase, 'cattle of the mountains.' It had been obscurely noticed (if indeed the same species of ox be meant,) by Knox, in his historical relation of *Ceylon**; and it has been imperfectly described by Captain Turner, in his journey through *Bootan*†. Herds of this species of cattle have been long possessed by many gentlemen, in the eastern districts of *Bengal*, and also in other parts of this province: but no detailed account of the animal, and of its habits, has been yet published in *India*. To remedy this deficiency, Dr. Roxburgh undertook, at my solicitation, to describe the *gayál*, from those seen by him in a herd belonging to the Governor General. Dr. Buchanan has also obligingly communicated his observations on the same cattle: and both descriptions are here laid before the society; with information obtained from several gentlemen at *Tipura*, *Silhet*, and *Chatgaon*, relative to the habits of the animal.

A distinct species.

From the information which was first received, it was supposed that the *gayál* would not engender either with the buffalo, or with the common bull and cow, and must therefore constitute a distinct species in every system of classification. Although this is contradicted by the correcter information now obtained, yet on account of the considerable, and apparently permanent, difference between the common cow and the *gayál*, this ought still, perhaps, to be considered as a distinct species, rather than as a variety.

Description by Dr. Roxburgh.

'The *gayál*,' says Dr. Roxburgh, 'is nearly of the size and shape of the *English* bull. It has short horns, which are distant at their bases, and rise in a gentle curve directly out and up: a transverse section, near the base, is ovate; the thick end of the section being on the inside. The front is broad, and crowned with a tuft of lighter coloured, long, curved hair. The dewlap is deep and pendent. It has no mane, nor hump; but a considerable elevation over the withers. The tail is short; the body covered with a tolerable coat of straight, dark-brown hair: on the belly, it is lighter coloured; and the legs and face are sometimes white.'

Doctor Buchanan thus describes it:

* P. 21.

† Embassy to *Tibet*, p. 160.

The

'The *gayál* generally carries its head with the mouth projecting forward like that of a buffalo. The head, at the upper part, is very broad and flat, and is contracted suddenly towards the nose, which is naked, like that of the common cow. From the upper angles of the forehead proceed two thick, short, horizontal processes of bone, which are covered with hair. On these are placed the horns, which are smooth, shorter than the head, and lie nearly in the plane of the forehead. They diverge outward, and turn up with a gentle curve. At the base they are very thick, and are slightly compressed, the flat sides being toward the front and the tail. The edge next the ear is rather the thinnest, so that a transverse section would be somewhat ovate. Toward their tips, the horns are rounded, and end in a sharp point. The eyes resemble those of the common ox; the ears are much longer, broader, and blunter than those of that animal. By Dr. Buchanan. Horns.

'The neck is very slender near the head, at some distance from which a dewlap commences; but this is not so deep, nor so much undulated, as in the *bos zebu*, or *Indian* ox. The dewlap is covered with strong longish hair, so as to form a kind of mane on the lower part of the neck; but this is not very conspicuous, especially when the animal is young. Neck.

'In place of the hump, which is situated between the shoulders of the *zebu*, the *gayál* has a sharp ridge, which commences on the hinder part of the neck, slopes gradually up till it comes over the shoulder joint, then runs horizontally almost a third part of the length of the back, where it terminates with a very sudden slope. The height of this ridge makes the neck appear much depressed, and also adds greatly to the clumsiness of the chest, which, although narrow, is very deep. The sternum is covered by a continuation of the dewlap. The belly is protuberant, but in its hinder part is greatly contracted. The rump, or *os sacrum*, has a more considerable declivity than that of the *European* ox, but less than that of the *zebu*. Body.

'The tail is covered with short hair, except near the end, where it has a tuft like that of the common ox; but in the *gayál*, the tail descends no lower than the extremity of the *tibia*. Tail.

'The legs, especially the fore ones, are thick and clumsy. Legs.
The

The false hoofs are much larger than those of the *zebu.* The hinder parts are weaker in proportion than the forehand; and, owing to the contraction of the belly, the hinder legs, although in fact the shortest, appear to be the longest.

Hair.

'The whole body is covered with a thick coat of short hair, which is lengthened out into a mane on the dewlap, and into a pencil-like tuft on the end of the tail. From the summit of the head there diverges, with a whirl, a bunch of rather long coarse hair, which lies flat, is usually lighter coloured than that which is adjacent, and extends towards the horns, and over the forehead. The general colour of the animal is brown, in various shades, which very often approaches to black, but sometimes is rather light. Some parts, especially about the legs and belly, are usually white, but in different individuals, they are very differently disposed.

Colour.

'In the first column of the following table is the measurement of a full grown cow: in the second is that of a young male.

Size.

	Ft.	*In.*	*Ft.*	*In.*
From the nose to the summit of the head	1	6	1	8
Distance between the roots of the horns	0	10	0	9
From the horns to the shoulder	3	3	3	0
From the shoulder to the insertion of the tail	4	3	3	10
Height at the shoulder	4	9	4	7
Height at the loins	4	4	4	2
Depth of the chest	2	9	-	-
Circumference of the chest	6	7	5	7
Circumference at the loins	5	10	5	6
Length of the horns	1	2	-	-
Length of the ears	0	10	-	-

Distinguishing characters of the species of ox.

'The different species of the ox kind may be readily distinguished from the *gayál* by the following marks. The *European* and *Indian* oxen by the length of their tails, which reach to the false hoofs; the *American* ox by the gibbosity on its back; the *boves moschatus, Cafer,* and *pumilus,* by having their horns approximated at the bases; the *bos grunniens* by its whole tail being covered with long silky hairs; the *bos bubalus,* at least the *Indian* buffalo, by having the whole length of its horns compressed, and by their being longer

longer than the head, and wrinkled; also by its thin coat of hair, by its want of a dewlap, and, above all, by its manners; the *bos barbatus* by the long beard on its chin.

'The cry of the *gayál* has no resemblance to the grunt of the Indian ox, but a good deal resembles that of the buffalo. It is a kind of lowing, but shriller, and not near so loud as that of the European ox. To this, however, the *gayál* approaches much nearer than it does to the buffalo.' Voice.

The result of inquiries made by Mr. Macrae, at *Chatgaon*, has been communicated by that gentleman, in the following answer to questions which were transmitted to him.

'The *gayál* is found wild in the range of mountains that form the eastern boundary of the provinces of *Aracan, Chittagong (Chatgaon), Tipura*, and *Silhet*. Native place.

'The *Cúcis, or Lunctas*, a race of people inhabiting the hills immediately to the eastward of *Chatgaon*, have herds of the *gayál* in a domesticated state. By them he is called *shïal*; from which, most probably, his name of *gayál* is derived; as he is never seen on the plains, except when brought there. By the *Mugs* he is named *j'hongnuah;* and by the *Burmas, núnec*. In the *Hindu s'ástra* he is called *gabay*. It appears, however, that he is an animal very little known beyond the limits of his native mountains, except to the inhabitants of the provinces abovementioned. Domesticated.

'The *gayál* is of a dull heavy appearance; but, at the same time, of a form which indicates much strength and activity, like that of the wild buffalo. His colour is invariably brown, but of different shades, from a light to a dark tinge; and he frequently has a white forehead, and four white legs, with the tip of the tail also white. He has a full eye, and, as he advances in age, often becomes blind; but it is uncertain whether from disease, or from a natural decay. His disposition is gentle; even when wild, in his native hills, he is not considered to be a dangerous animal, never standing the approach of man, much less bearing his attack. The *Cúcis* hunt the wild ones for the sake of their flesh. Appearance. Habits.

'The *gayál* delights to range about in the thickest forests, where he browses, evening and morning, on the tender shoots

shoots and leaves of different shrubs; seldom feeding on grass, when he can get these. To avoid the noonday heat, he retires to the deepest shade of the forest; preferring the dry acclivity of the hill to repose on, rather than the low swampy ground below; and never, like the buffalo, wallowing in mud.

Milk. 'The *gayál* cow gives very little milk, and does not yield it long; but what she gives is of a remarkable rich quality, almost equally so with the cream of other milk, and which it also resembles in colour. The *Cúcis* make no use whatever of the milk, but rear the *gayáls* entirely for the sake of

Hide. Flesh. their flesh and skins. They make their shields of the hides of this animal. The flesh of the *gayál* is in the highest estimation among the *Cucis*; so much so, that no solemn festival is ever celebrated without slaughtering one or more *gayáls*, according to the importance of the occasion.

Turned loose. 'The domesticated *gayáls* are allowed by the *Cúcis* to roam at large during the day through the forest in the neighbourhood of the village; but, as evening approaches, they all return home of their own accord; the young *gayál* being early taught this habit, by being regularly fed every

Fond of salt. night with salt, of which he is very fond: and, from the occasional continuance of this practice, as he grows up, the attachment of the *gayál* to his native village becomes so strong, that, when the *Cúcis* migrate from it, they are obliged to set fire to the huts which they are about to leave, lest their *gayáls* should return thither from their new place of residence, before they become equally attached to it, as to the former, through the same means.

Food. 'The wild *gayál* sometimes steals out from the forests in the night, and feeds in the rice fields bordering on the hills. The *Cúcis* give no grain to their cattle. With us, the tame *gayáls* feed on *caláï (phaseolus max.)*; but, as our hills abound with shrubs, it has not been remarked what particular kind of grass they prefer.

Another species wild. 'The *Hindus*, in this province, will not kill the *gabay*, which they hold in equal veneration with the cow. But the *as'l gayál*, or *seloï*, they hunt and kill, as they do the wild buffalo. The animal here alluded to is another species of *gayál* found wild in the hills of *Chatgaon*, a correct description

description of which will be given hereafter. He has never been domesticated; and is, in appearance and disposition, very different from the common *gayál*, which has been just described. The natives call him the *as'l gayál* in contradistinction to the *gabay*. The *Cúcis* distinguish him by the name of *seloí*, and the *Mugs* and *Barmas* by that of *p'hanj*; and they consider him, next to the tiger, the most dangerous and the fiercest animal of their forests.' and very fierce.

Mr. Elliott says: 'I have some *gayáls* at *Munnamutty*, and, from their mode of feeding, I presume, that they keep on the skirts of the vallies, to enable them to feed on the sides of the mountain, where they can browse. They will not touch grass, if they can find shrubs. Browse.

'While kept at *Camerlah*, which is situate in a level country, they used to resort to the tanks, and eat on the sides, frequently betaking themselves to the water, to avoid the heat of the sun. However, they became sickly and emaciated, and their eyes suffered much; but, on being sent to the hills, they soon recovered, and are now in a healthy condition. They seem fond of the shade, and are observed in the hot weather to take the turn of the hills, so as to be always sheltered from the sun. They do not wallow in mud like buffaloes; but delight in water, and stand in it, during the greatest heat of the day, with the fronts of their heads above the surface. Require a hilly country.

I take this opportunity, while treating of a species of ox, to notice an errour which crept into Kerr's unfinished translation of the animal kingdom in Linnæus's Systema Naturæ; and which has been followed by Doctor Turton in translating the general system of nature by Linnæus. Mr. Kerr described and figured, under the name of *bos arnee*, an animal, which, notwithstanding the exaggerated description, given on the authority of 'a British officer, who met with one in the woods, in the country above *Bengal**,' is evidently nothing else but the wild buffalo, an animal very common throughout *Bengal*, and known there, and in the neighbouring provinces of *Hindostan*, by the name of *arna*. Though neither fourteen feet high, as Mr. Kerr has Mistake of Kerr and Turton.

* Kerr, page 336.

stated,

stated, or rather as the officer, on whose information he relied, had affirmed; nor even eight feet high, as Doctor Turton, following Kerr's inference from a drawing, asserts; yet it is a large and very formidable animal, conspicuous for its strength, courage, and ferocity. It may not be true, that the buffaloes of *Asia* and *Europe* constitute a single species; but, certainly, the wild and tame buffaloes of *India* do not appear to differ in any thing, except the superior size and more uniform figure of the wild animal. A better description of the buffalo, than has yet been given, is perhaps wanted; but the *bos arnee* of Kerr and Turton must be rejected from systems of *zoology*, as an erroneous description taken from a loose drawing, assisted by the fragment of a skeleton.

Good description of the buffalo wanted.

IX.

A concise Method of determining the Figure of a gravitating Body revolving round another. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Deficiency of Newton's researches.

IT is well known, that there are some imperfections in Sir Isaac Newton's investigations respecting the figures of gravitating bodies, which have been supplied by Maclaurin and Clairaut: the subject is however still considered as difficult and intricate, and the simplest calculations respecting it have hitherto been too prolix, to be distinctly conceived as links of the same chain. I shall endeavour to point out a method of treating it which is extremely compendious and convenient.

Forces concerned.

Neglecting in the first place the diurnal rotation, we may suppose, that each particle of the body revolves in an equal orbit, so that its centrifugal force may be equal to the mean attractive force; then the local attractive force will be greater or less than this by a difference which must obviously be proportional to the distance from an equatorial plane perpendicular to the direction of the central body, and tending to

to remove the body from this plane. A second disturbing force will also arise from the want of parallelism in the direction of the attractive force, which will tend towards the line joining the centres, and will be every where to the whole force as the distance from this line to the distance of the bodies. Now if each of these forces be reduced to the direction of the circumference of the sphere, from which the figure is supposed to vary but very little, it will be every where proportional to the product of the sine and cosine of the distance from the equatorial plane, and when this distance is half a right angle, each of them will be half as great as in its intire state. Thus the gravitation towards the moon at the earth's surface is to the gravitation towards the earth as 1 to 70 times the square of $60\frac{1}{2}$, or to 256217, and the first disturbing force is to the whole of this as 2 to $60\frac{1}{2}$, at the point nearest to the moon, and the second as 1 to $60\frac{1}{2}$ at the equatorial plane; and the sum of both reduced to the direction of the circumference where greatest, as 3 to 121, that is, to the whole force of the earth's gravitation as 1 to 10,334,000. And in a similar manner the joint disturbing force of the sun is to the weight as 1 to 25,736,000.

Inclination of the surface of a spheroid.

Now if a sphere be inscribed in an oblong spheroid, the elevation of the spheroid above the sphere must obviously be proportional, if measured in a direction parallel to the axis of the spheroid, to the ordinate of the sphere, that is, to the sine of the distance from its equator; and if reduced to a direction perpendicular to the surface of the sphere, it must be proportional to the square of that sine; and the tangent of the inclination to the surface of the sphere, which is as the fluxion of the elevation divided by that of the circumference, must be expressed by twice the continual product of the sine, the cosine, and the ellipticity or greatest elevation, the radius being considered as unity: so that the ellipticity will also express the tangent of the inclination where it is greatest; and the inclination will be every where as the product of the sine and cosine.

Tides of a sea of inconsiderable density.

If therefore the density of the elevated parts be considered as evanescent and their attraction be neglected, there will be an equilibrium when the ellipticity is to the radius as the disturbing force to the whole force of gravitation: for each

 particle

particle situated on the surface will be actuated by a force preceisely equal and contrary to that which urges it in the direction of the inclined surface. Hence, if the density of the sea be supposed inconsiderable in comparifon with that of the earth, the radius being 20,839,000 feet, the height of a solar tide in equilibrium will be 2·0166 feet, and that of a lunar tide ·8097.

Attraction of the elevated parts.

We must next inquire what will be the effect of the gravitation of the elevated parts, on any given supposition respecting their density. Let us imagine the surface to be divided by an infinite number of parallel and equidistant circles, beginning from any point at which a gravitating particle is situated, and let their circles be divided by a plane bisecting the equatorial plane of the spheroid; it is obvious that if the elevations on the opposite sides of this plane be equal in each circle, no lateral force will be produced; but when they are unequal, the excess of the matter on one side above the matter on the other will produce a disturbing force. The elevation being every where as the square of the distance from the equatorial plane, the difference, corresponding to any point of that semicircle in which the elevation is the greater, will be as the difference of the squares of the distances of the corresponding points of the two semicircles, that is, as the product of the sum and the difference of the distances: but the sum is twice the distance of the centre of the circle from the equatorial plane, or twice the sine of the distance of the gravitating particle from the plane, reduced in the ratio of the radius to the cosine of the angular distance of the circle from its pole; and the difference is twice the actual sine of any arc of the circle, reduced to a direction perpendicular to that of the plane, that is, reduced in the proportion of the radius to the cosine of the angular distance of the given particle from the equatorial plane. From these proportions it follows, that, in different positions of the gravitating particle, the effective elevation at each point of the surface, similarly situated with respect to it, is as the product of the sine and cosine of its angular distance from the equatorial plane, the other quantities concerned remaining the same in all positions: the disturbing attraction of all the prominent parts varies therefore precisely

cisely in this ratio, the matter which produces it being always similarly arranged, and varying only in quantity; consequently the sum of this attraction and the original disturbing force both vary as the inclination of the surface, and may be in equilibrium with the tendency to descend towards the centre, provided that the ellipticity be duly commensurate to the density of the elevated parts.

Magnitude of the ellipticity.

In the last place we must investigate what is the magnitude of the ellipticity corresponding to a given disturbing force and a given density. It follows from the proportions already mentioned, first, that the effectual elevation at each point of each concentric semicircle is proportional to the sine of its distance from the bisecting plane; and secondly, that the greatest effective elevation of each semicircle, for any one position of the superficial particle, is as the product of the sine and the cosine of the angular distance from that particle, the diameter of the circle being as the sine, and the distance of its centre from the equatorial plane as the cosine. It may easily be shown, that the disturbing force, reduced to the direction of the surface, or of the plane of each circle, is equal to the attraction which would be exerted by the matter covering the whole semicircle to a height equal to half the greatest elevation, if placed at the middle point: for the elevation being as the sine of the distance from the bisecting plane, and the comparative effect being also as the sine, the attraction for each equal particle of the semicircle is as the square of the sine, and the whole sum half as great as if each particle produced an equal effect with that on which the elevation is greatest. We must therefore compute the attraction of the quantity of matter thus determined, supposing it to be disposed at the respective points of a great circle passing through the given point and the pole of the spheroid. The immediate attraction of each particle being inversely as the square of the chord, its effect reduced to the common direction will be as the sine directly, and the cube of the chord inversely, and this ratio being compounded with that of the product of the cosine and the square of the sine, which expresses the quantity of matter at each point, the comparative effect will be as the cube of the sine and the cosine directly, and as the cube of the

chord inversely, or as the cube of the cosine of half the arc and the cosine of the whole arc conjointly. If therefore we call the cosine of half the arc x, the cosine of the whole arc will be $2x^2 - 1$, and the fluxion of the arc being $-\frac{2\dot{x}}{\surd(1-xx)}$, that of the force will be $\frac{2x^3\dot{x} - 4x^5\dot{x}}{\surd(1-xx)}$, of which the fluent is $(\frac{4}{5}x^4 + \frac{2}{5}x^2 + \frac{4}{5})\ \surd(1-xx)$, as may be shown by substituting, in the reduction of its fluxion, $\frac{1-xx}{\surd(1-xx)}$ for $\surd(1-xx)$: and while x decreases from 1 to 0, this fluent becomes $\frac{4}{5}$. But in order to determine the unit with which this quantity is to be compared, we must consider the initial force as unity, and imagine, that it is continued through an arc equal in length to the radius; and we must find the attraction of the solid contained between a circular plane and a conical surface, initially touching the effective portion of the elevation, and including it between them; the attraction reduced to a common direction, being initially half the whole attractive force of such a solid, as we have already seen of the concentric circles considered separately. But the attraction of any slender conical or pyramidical body for a particle placed at its vertex, is three times as great as that of the same quantity of matter situated at its base; consequently the attraction of the supposed solid is equal to that of the circumscribing semicylinder placed at the distance of the radius: the conical excavation being half of the solid, and the semicylinder triple of the cone: but the height of this semicylinder in the case of a particle situated half way between the pole and the equator of the spheroid, is twice the ellipticity, the tangent of the angle of mutual inclination of the surfaces of the effective elevation being initially equal to twice the greatest ordinate, because the product of the sine and cosine, when greatest, is equal to half of the radius: the semicylinder will therefore be equal to a cylinder of which the diameter is equal to that of the sphere, and the height equal to the ellipticity; and the contents of this cylinder will be to that of the sphere, as $\frac{3}{4}$ of the ellipticity to the radius. Such therefore is the unit with which the disturbing attraction is to be compared; and when the densities

sities are equal, this force will be to the whole weight as $\frac{3}{4}\cdot\frac{4}{5}$ or $\frac{3}{5}$ of the ellipticity to the radius; and the portion of the inclination remaining to be compensated by the primitive disturbing force will be $\frac{2}{5}$ of the whole, so that the ellipticity must be to the proportional disturbing force as 5 to 2. And if the density of the sea be to the mean density of the earth as 1 to n, the disturbing force, produced by its attraction, will be to the ellipticity as $\frac{3}{5n}$ to 1, and the primitive disturbing force as $1-\frac{3}{5n}$ to 1.

Tides of a homogeneous spheroid, and of the sea as it actually exists.

The heights of the solar and lunar tides in equilibrium having been found equal to ·8097 and 2·0166 feet respectively, on the supposition of the density of the sea being inconsiderable, they must be increased to 2·024 and 5·042 for an imaginary planet of uniform density; but since n is in reality about $5\frac{1}{2}$, and $\frac{3}{5n}$ nearly $\frac{1}{9}$, the ellipticity must be to the primitive disturbing force only as 1 to $\frac{8}{9}$ or 9 to 8, and the height of the sides in equilibrium ·911 and 2·269 respectively, and the joint height 3·18 feet. And when the surface assumes any other form than that which affords the equilibrium, the force tending to restore that form is always less by one ninth than it appears to be when the attraction of the elevated parts is neglected. The theory of the tides must therefore be very materially modified by these considerations, although they do not affect the general method of explaining the phenomena.

Ellipticity from centrifugal force, and density of the superficial parts of the earth.

These calculations are also immediately applicable to the figure of an oblate spheroid: for it may easily be shown, that the difference of the elevations in the opposite halves of each semicircle is precisely the same in an oblate as in an oblong spheroid of equal ellipticity: so that the ellipticity must here also be to the disturbing force, where it is greatest, as 1 to $1-\frac{3}{5n}$, or to the centrifugal force at the equator as 1 to $2-\frac{6}{5n}$. Thus, the centrifugal force being $\frac{1}{289}$, if the density were uniform, the ellipticity would be $\frac{1}{231}$; but since it is in reality about $\frac{1}{315}$, $2-\frac{6}{5n}=\frac{315}{289}$, and $n=1\cdot32$,

$n = 1{\cdot}32$, n implying here the mean density of the earth compared with the mean density of the elevated portion of the spheroid, which hence appears to be about three fourths of that of the whole earth. It is obvious that, in this case as well as in the former, if the density of the sea were two thirds greater than that of the earth, the slightest disturbing force would completely destroy the equilibrium, and the whole ocean would be collected on one side of the earth.

I am, Sir,

Your very humble servant,

A. B. C. D.

X.

Description of a new Compensation Pendulum; by Lieutenant HENRY KATER. *Communicated by the Author.*

Many attempts to correct the irregularity of clocks from heat and cold.

SINCE the first application of the pendulum to clocks, numerous attempts have been made, to correct the errour arising from a variation of temperature, which, by contracting or dilating the substance of which the pendulum rod is composed, occasions the clock to go faster in cold than in warm weather, and consequently to vary considerably in its rate at different seasons of the year.

Defects of the gridiron pendulum,

The gridiron pendulum, now used in almost all regular observatories, though generally supposed to be the best calculated to remedy this inconvenience, is complex, and requires the greatest nicety in proportioning the brass and steel rods (of which it is composed) to each other; it is very expensive; and, if it should be badly constructed at first, is incapable of adjustment.

and of the mercurial.

The mercurial pendulum is not so liable to the last of these objections, for, if the quantity of mercury should not be found exactly to compensate for the expansion of the metallic rod, a little may be added or taken away; but then it must require very numerous trials, before an accurate result can be obtained.

A simple,

These considerations induced me, to turn my attention to the

the subject, and to endeavour to construct a pendulum, which should unite simplicity and cheapness, with the capability of being easily and accurately adjusted.

cheap, and easily, adjusted pendulum desirable.

Wood has been long known as a substance that expands less than any other with heat; and from this property many pendulum rods of time-pieces have been made of wood, and found to answer remarkably well; but it is surprising no advantage has hitherto been taken of this knowledge, to apply a compensation, which might counteract the small expansion to which a pendulum of wood is liable.

Wood advantageous, but no attempt yet to compensate its expansion.

Wood therefore, if it can be rendered perfectly impervious to moisture, appears to be by far the best material, that can be used for the *rod* of the pendulum; and as zinc is a metal which suffers the greatest expansion from an increase of temperature, I consider it preferable to every other, that could be employed as a compensation.

Wood best for the rod, with zinc as a compensation.

The first step was to ascertain accurately the quantity of the expansion of wood, as I could find no experiments on the subject at all satisfactory. For this particular purpose a pyrometer was used, which it would be unnecessary here to describe, calculated to receive a rod of wood four feet in length, one end of which was made to act against the shorter arm of a lever, causing the longer to describe an arc, the divisions of which might easily be read off to the thousandth part of an inch.

Expansibility of wood examined.

A rod of very dry and well seasoned white deal was procured, free from knots, four feet in length, three quarters of an inch in breadth, and a quarter of an inch thick. Each end was exactly squared, and covered with a thin flat plate of brass. This rod was exposed in an oven to the temperature of 235°, and on measuring it in the pyrometer, it was found to have contracted; it was therefore replaced in the oven, and suffered to remain a long time till it appeared a little discoloured, in order to dissipate all moisture. The temperature of the oven was then examined, and found to be still 235°. The deal rod was now quickly removed, and placed in the pyrometer, where it remained a sufficient time to acquire the temperature of the room, which was 49°, when the space described in the interval by the long arm of the lever, was registered; and in this manner by two experiments,

Rod of white deal $\frac{3}{4}$ inch broad and $\frac{1}{4}$ thick

expanded •0049 inch per foot with 180° of heat.

ments, which gave precisely the same result, the expansion of 4 feet of white deal was determined to be 0·0205 parts of an inch with 186° of Fahrenheit's thermometer, from which by proportion we get 0·0049 parts of an inch for the expansion of one foot with 180° difference of temperature.

I shall now attempt to give a general description of the pendulum, and then proceed to a more particular account of the manner in which it is constructed.

Pendulum described.

A B C D, Pl. V, fig. 1, is cast in zinc. From A to B is one inch; and from C to D nearly two inches: The height of A C is ten inches. Above A B a piece of brass is soldered, an inch square and half an inch thick, through which a hole is made four tenths of an inch in diameter, and tapped with a very fine screw. A cylinder of zinc, E F, about two inches and a half in length, has a screw on it to fit that in the piece of brass as accurately as possible. This cylinder should be carefully turned in a lathe, on a hole as a centre about the eighth of an inch diameter, and made quite through it; the top of the cylinder to the length of a quarter of an inch is filed square, for the purpose of more readily turning the screw with a key, or pincers: and there is a thin plate of brass, represented at *c, d,* which screws on the cylinder, in order to fix it firmly at any height.

In the bottom plate of zinc, C D, a hole is made half an inch in diameter, through which the pendulum spring passes, and the whole is fastened by four screws to the cock of the time-piece, which is represented by fig 2, and which is cast for the purpose with the addition of a plate of brass on which the compensation rests.

A steel wire, *g, h,* with a fine screw on it, passes through the hole in the cylinder of zinc, by means of which, and the nut below *g,* the pendulum is shortened or lengthened. The watch spring, which supports the pendulum, is fastened to the steel wire at *h,* by means of a pin, and, passing through a slit in the plate of brass on which the compensation rests, is attached to the end of the pendulum rod in the manner hereafter described.

The *rod* of the pendulum is made of white deal, three quarters of an inch broad, and four tenths of an inch thick, and is chosen perfectly free from knots, and well-seasoned.

Previous

Previous to its being reduced to the exact dimensions, it is to be baked in an oven, till the surface appears a little charred; and as it is of the utmost importance, that it should be rendered perfectly impervious to moisture, the ends are soaked in melted sealing wax, and the rod, being cleaned, is coated several times with copal varnish.

Pendulum described.

The top of the rod is to be divided with a very fine saw, to admit the spring of the pendulum, where it is secured by two or three small pins passing through it and the spring, and rivetted on each side.

The *weight* of the pendulum is of the usual form, and pierced to receive the rod, which is immovably fixed to the centre by means of a screw, passing through it and the weight.

The length of the pendulum is regulated by the screw and nut at the top; but there is also a screw with a less weight at the lower extremity of the pendulum, in order to adjust it with greater accuracy in the usual manner.

Now it is evident, that, if the part which is made of zinc be so proportioned to the other materials of which the pendulum is composed, as to undergo an equal expansion with any increase of temperature, the pendulum will always maintain the same length, and its oscillations, as far as temperature is concerned, will be performed in equal times.

In order to discover the length of zinc necessary to effect this, let the steel screw, *g*, *h*, be 9·5 inches long; and the spring 3 inches, making together 12·5 inches. Then, as the expansion of zinc to steel is as 353 to 147, we shall have $\frac{147 \times 12{\cdot}5}{353} = 5{\cdot}2$ inches nearly, for the length of zinc required to correct the expansion of the *steel* employed in the pendulum. Next, as the spring will extend about two inches below the plate of the cock of the time-piece, whence the length of the pendulum is measured, there will remain about 37 inches of deal, and the expansion of deal being to that of zinc as 49 to 353, we have $\frac{49 \times 37}{353} = 5{\cdot}1$ inches nearly; which, being added to 5·2 before found, gives 10·3 inches, the length of zinc which will counteract the expansion of the whole pendulum.

Calculation of the length of zinc necessary for the compensation.

The

The length of the zinc may be altered.

The length of the zinc may be varied by means of the screw EF, and experience alone can determine, whether the compensation be accurate, for, if the clock be found to *gain* in warm weather, it is evident, that the zinc is too long, and to correct it the screw E F, must be advanced; and vice versa.

Principles of its adjustment.

The quantity of the alteration requisite may be very nearly determined by knowing how much the clock has varied from its regular rate during a certain period, the difference of temperature, and the measure of one revolution of the screw E F, which last should be previously ascertained with the utmost accuracy. It is scarcely necessary to add, that the pendulum must be as much raised by means of the nut below *g*, as it was lowered by shortening the compensation.

Another mode of applying zinc as a compensation to a wooden rod.

There is another mode of applying a compensation of zinc to a rod of wood, far more simple than that already described; but it appears liable to some objections, which experience may perhaps prove to be unfounded. The rod is made of deal prepared in the manner before mentioned, but is suspended from the cock of the time-piece in the common way by a spring one inch in length.

This compensation described.

A square tube of zinc, represented at fig. 3, is cast seven inches long, and three quarters of an inch square; the *internal* dimensions of the tube are four tenths of an inch each side. The lower part of the pendulum rod remains of the same *thickness*, but is cut away on the two sides for the length of seven inches, so as to slide with perfect freedom in the tube of zinc. A piece of brass, rather more than a quarter of an inch thick, is soldered to the bottom of the tube of zinc at C; and a hole with a fine screw is made through it similar to that before described in fig. 1. A cylinder of zinc of the same description as E F, fig. 1, but only an inch and a half in length, is made to screw into the piece of brass just mentioned, and a thin plate of brass screws on the cylinder to prevent any shake after the length of zinc necessary for the compensation shall have been accurately determined.

In the two opposite sides of the zinc tube two small grooves (*a*, *b*, fig 3) are made with a file at about the eighth of an inch from the top of the tube, and parallel to it, and about the twentieth of an inch in depth.

A piece

A piece of iron, is forged one foot long, half of which is made of the same size as the zinc tube is *externally*, and the remainder of the same dimensions as the *upper part* of the pendulum rod. This iron being placed between the plates of brass, which form the weight of the pendulum, the lead is cast around it, and the iron is afterward forced out, leaving a receptacle for the tube of zinc, which extends *exactly* to the centre of the weight.

This compensation described.

The brass plate at the back of the weight of the pendulum being taken off, a square opening (the upper part of which is in a line with the centre of the weight,) is cut through the lead into the receptacle for the tube of zinc; and the tube having been made to slide with perfect freedom, yet without any shake in the receptacle, it is passed up as far as it will go; and marks being made opposite the groves *a*, *b*, fig. 3, as represented at fig. 4, corresponding groves are made in the lead with a gouge of the proper size, into which a small quantity of melted lead being poured, the tube of zinc remains firmly secured, and of course can expand in no other direction than from the centre of the weight downwards.

In order to restore the lead, which was taken away in making the square opening; a piece of card paper is cut so as to cover the zinc, and the opening may then be filled with melted lead (being careful that it is not too hot,) without any danger of its uniting with the tube: the back plate of the pendulum weight is then to be replaced and rivetted.

To the lower end of the pendulum rod a cap of brass is firmly fixed, from the bottom of which a strong steel screw proceeds, which passing through the hole in the crylinder of zinc, the weight is supported, and the pendulum regulated, by a nut in the usual manner; and a small octagonal plate of brass is soldered to the bottom of the cylinder, to prevent the nut from injuring the zinc, as well as to divide each revolution of the screw into eight equal parts.

Determination of the length of zinc for this compensation.

To determine the length of zinc required for a compensation of this form, that part of the steel screw included between the nut and the end of the deal rod must be considered. This we will suppose to be two inches, which will leave half an inch, by which the length of the pendulum may be varied:

ried: adding this to one inch, the length of the spring by which the pedulum is suspended, we have three inches of steel; and the expansion of steel to zinc being as 147 to 353, we have $\frac{147 \times 3}{353} = 1{\cdot}25$ inches nearly for this part of the correction.

The deal rod will be about 44·5 inches long; and its expansion being to that of zinc as 49 to 353, we have $\frac{49 \times 44{\cdot}5}{353} = 6{\cdot}17$ inches nearly for the length of zinc necessary to counteract the expansion of the deal, which being added to 1·25 inches, before found, gives 7·42 inches for the whole compensation sought.

Mode of adjustment.

The adjustment is effected in the same manner as before described by means of the screw E F, by which the length of the zinc is either increased or diminished; and below the large weight is a smaller one, for the purpose of regulating the pendulum to the greatest nicety. This small weight may have a tube of zinc attached to it, on the same principle as that of the larger, to correct the expansion of the steel screw, if it be thought necessary.

Objection to this compensation.

The chief objection to this pendulum appears to be, that the compensation is partly enclosed in the weight, and consequently is not likely to be so soon affected by any sudden variation of temperature, as it would be if it were exposed to the immediate influence of the atmosphere.

Its advantages.

But it has the advantage of being much shorter, and far more simple in its construction, than the one first described, and is therefore on the whole perhaps preferable.

The compensation may be divided.

If it be thought more convenient, the compensation may be divided, and half placed between the weight of the pendulum, and the other half on the cock of the time-piece; and the nut for regulating it may be either above or below.

Advantages of this pendulum.

Experiments in regular observatories can alone determine the relative merits of this pendulum; It certainly possesses the superior advantages of economy, simplicity, and ease of adjustment, and there appears every reason to believe, that it may be found at least equal in point of accuracy to any that has hitherto been described.

Exeter, April, 1808.

XI.

Extract of a Letter from Mr. J. ASTON *of Ipswich, giving an Account of a Mule Cucumber, and other Objects.*

I Have taken the liberty of sending you a curious production of nature, which was produced in the following manner; Mr. Chapman, the proprietor of very extensive pineries in this town, had growing in one of his hot houses a plant of the cucumis colocynthis (coloquintida, or bitter apple), which happened to put forth a male blossom a day or two before it was removed into the open air. In the same house there were also growing some plants of the common cucumber also in blossom at some distance from the other plant. It is supposed some of the farina was carried by a bee from the blossom of the coloquintida to a female one of those on the cucumber, which thus became impregnated, and produced the fruit I send you. Mr. Chapman says he noticed the cucumber when about an inch and a half or two inches long, and it had every appearance of becoming a very fine fruit, but soon afterward it began to swell, and continued to do so till the other day, when he gathered it and presented it to me.

Curious natural production.

Farina of the coloquintida impregnated a common cucumber.

I had some thoughts of sending you a drawing of it, but, as I am a very indifferent botanist, it struck me, that you would not be able so well to understand its nature either by delineation or description, as by seeing the fruit itself. Mr. Chapman is an intelligent man, and has been for many years engaged in horticultural pursuits*. You will perceive it is what is called a mule fruit partaking of the nature of both the parent plants. See Pl. V, fig. 5.

If you should be of opinion, that it is a circumstance worth mentioning in your Journal, I beg you will do it in any manner you please, and in a way that you think will be most easily comprehended by botanists, to whom most probably the communication will be found acceptable.

* This Effect, I am informed is not unfrequent, and is ascribed to bees. Whole beds of melons have in some instances been thus spoiled. N.

When

Pyrites on the shore at Harwich.

When I was in London you may perhaps recollect I mentioned to you, that considerable quantities of iron pyrites were to be found upon the sea shore at Harwich. I have embraced the present opportunity of sending you a small specimen for your inspection. It would be curious to ascertain the true theory of its formation. From the little observation I have had an opportunity of making, I am persuaded its formation is considerably aided by the seawater. Pieces of wood, bone, &c. become converted into it by time, and lose every trait of their origin, except the shape of the grain, which in many specimens is nicely preserved. The cliff above the shore appears to be almost entirely composed of a blueish soft clay, which is continually crumbling and falling down upon the beach, and is washed by the waves, and I think a curious observer conversant in mineralogy might easily trace the formation of the pyrites by gradation from the clay, as pieces may be found in several different states, and it appears to be influenced by the alternate action of the air and sea water, but in what way I am entirely at a loss at present to conjecture.

Seawater appears to assist in its formation, converting wood, &c. into pyrites.

Ipswich, 6th of June, 1808.

XII.

Letter from Professor VINCE, *in Reply to* DYTISCUS.

To Mr. NICHOLSON.

SIR,

Two mistatements in the last letter of Dytiscus.

I Shall esteem it a favour, if you will insert a few remarks on the observations of Dytiscus in your last Journal, engaging not to trouble you again on this subject.

In the first paragraph there are two *unaccountable* misrepresentations, for I would not charge Dytiscus with doing it wilfully. He says, "the two first terms of the series very possibly allude to the two first terms of the *only* TWO series which are to be found in the essay, these two terms having been already mentioned *as sufficient for determining the force.*" Now I have put down the first terms of THREE of the

the series, with + &c., meaning, of course, that the other series and terms were to be supplied, as I before remarked, and which it is strange Mr. D. should have forgotten. And secondly, I have never mentioned, nor was it possible I could mention, that the two terms alluded to are sufficient to determine the force: a further proof with what little attention Dytiscus has read the essay.

Again, he says, "the series $\frac{\alpha}{a^2}+\frac{\beta}{a^4}+\frac{\gamma}{a^6}+$ &c. may *certainly* vary as $\frac{1}{a^2}$, if all the Greek letters after the first become inconsiderable, and our author has virtually confessed in his essay, that they do become inconsiderable." The series certainly can *not* vary as $\frac{1}{a^2}$. The quantities β, γ, &c. are very small, but still finite, and can only be rejected in an *approximation* to the law of force. The law of gravity varies *accurately* as $\frac{1}{a^2}$, and the series can never give that law, as I have proved in Art. 11.

An assertion of his erroneous.

Farther: "As to the difficulty of extending the law to the internal parts of the sun's substance, *it is perfectly obvious*, that the law of density, as well as that of the force, must be supposed to change at the surface of every material body, long before $\frac{Q}{a}$ can become equal to P." *Not* perfectly obvious. When we discover *sudden* variations of the laws of nature, it is not that the *primary* cause is necessarily altered, but that some of the circumstances under which it acts are changed, as in the present instance. Without considering the cause, we know, that the attractions of every two particles of matter composing the sun's body vary inversely as the squares of their distances, and at the same time constitute a whole force, which, to a body *external* to the sun, varies as $\frac{1}{a^2}$, and to an *internal* body, as a. It is not therefore necessary, that the law of attraction of the constituent particles should vary, in order to produce these different laws of force. According to Newton, *any* two particles

Another contradicted,

ticles of matter, either both within a body, or one within and the other without, tend toward each other by the same law of force, and therefore the *cause* of that tendency, that is, in our present consideration, the *variation* of the density of the fluid, must in both cases be regulated by the same law. If we were to admit the position advanced by Dytiscus, it would involve Newton in a contradiction, and instead of affecting the truth of my proposition, would further tend to confirm it. To change the law of density *immediately*, would be to substitute two fluids instead of one, such a change necessarily implying a change of the fluid; for what better criterion have we of different fluids, than that their constitutions are regulated by different laws? To defend his objections, Dytiscus makes an assumption totally inconsistent with Newton's hypothesis.

which would involve Newton in a contradiction.

I do not think it necessary to make any farther remarks on the observations of Dytiscus, and I must make an apology to mathematicians for having said so much; but I was induced to do it upon this consideration, that they might not mislead those who are ignorant of the subjeet.

I am, Sir,

Your obliged humble servant,

Cambridge, 9 *June*, 1808. S. VINCE.

XIII.

Certain Improvements in Chronometers, by DANIEL DERING MATHEW, *Caius College, Cambridge. In a Letter from the Author.*

SIR,

Chronometers have been greatly improved.

THE degree of accuracy, to which chronometers have been brought within these few years, may appear to be the utmost to which, in a machine so complicated, human art could extend; but as navigation has derived great advantages from improvements made in them, I have been tempted to make some alterations in their construction, the superiority

superiority of which I leave for your candid readers to decide.

The principle of Mr. Mudge's free escapement (see 4to Journal, vol. ii, p. 56) is, I believe, allowed to be the best that was ever offered to the public; but its performance has not been found to be superior to the others, most likely on account of there being so many pivots and springs, and on account of its tripping, whence it cannot be depended upon. Mr. Arnold says, he has made his pendulum spring so, that the vibrations are performed in the same time when the main spring is weak, as when it is strong. This perhaps may be in some degree accomplished by very fine workmanship, and a great many trials, but the main spring is not detached from the balance; and on this account I think the title of being detached is not correct, as the main spring keeps up the action of the balance.

Mudge's scapement.

Arnold's.

My alterations and improvements, if I may so call them, consist, 1st, In reducing the wear and friction of Mr. Mudge's escapement, and putting it into a more simple form. 2dly, In applying my equalizing maintaining powers in such a manner, that tension does not alter their strength. 3dly, In securing the locking of the tooth against the detent. 4thly, In stopping the holes with hard platina.

Principles of the author's.

For these purposes I have two escapement wheels, equal and similar in all respects, as seen in Pl. VI, fig. 1. *a* A, *b* B, *c* C, *d* D, *e* E, *f* F, represent the teeth of the two wheels, which are so placed, that the tooth A of the upper wheel is exactly between the two teeth *a b* of the lower wheel. These teeth are prevented from revolving round by the two detent pallets G H, which turn on a pin, and concentric with these detent pallets the pivots of the verge turn, which is in the form of a crank as at M, or more plainly at fig. 2. *y y* are two joints at the ends of each of the arms of the pallets G H, in which the pieces *x x* are screwed, so as to allow a free motion. These pieces are fixed to the ends of two springs K L, which are made similar to the main spring in a gun lock. Each of these springs turns upon a stud *m*, as seen at K, fig. 3; and the spring is made stronger or weaker by the regulating screw *n*. The stud *m* is made of brass and the screw is steel, therefore the

Description of it.

the greater expansion of the stud will in some measure counteract the alterations of the springs by heat and cold. N N is the potance.

Its manner of acting.

The action of the escapement is thus. The wheel being propelled by the main spring in the direction of the arrow V, and prevented from revolving by the detent of the pallet H, the balance by its vibration knocks out the detent at *d*, and at the same time the tooth E raises the pallet G, till it comes to its detent. In the mean time the balance carries the pallet H through its semivibration, and is followed back by the pallet as far as the rim of the wheels between the teeth *d e*, this gives the balance force sufficient to knock out the detent of the pallet G, and the same action and reaction will continue so long as the moving power acts.

To prevent any possibility of the wheel tripping, I put two banking pins as at *g* on the arm of each pallet, which prevent the pallet from going farther back than is necessary to allow the tooth to raise up the pallet to its detent, by means of the catches *p p*, the end of which is a fine tender spring; and I make a circular piece to project out from the catch, so that the crank in its vibration first raises up this catch, and keeps it up while it knocks out the detent.

Advantages of this scapement.

Having explained the action of my scapement, I will now state a few of the advantages, which appear to me to arise from these alterations. 1st. By making use of a double wheel, I not only reduce the wear of the teeth, but I can in this way place my detent pallets and back springs so as not to interfere with one another, and I can have the pallets to turn upon the same centre. 2d. Straight springs are always preferable to a spiral one, where they can be used, because they are not so difficult to make, and their strength can be altered by adjusting screws, which cannot be done when spiral springs are used. Another advantage gained by using straight springs is, that the compensation may be put to the springs themselves, which is preferable to a compensation on the balance.

Objections answered.

In using a gun lock spring, the pressure of the tooth against the inclined plane is equal, and will therefore wear the face of the pallet equally. I am aware, that many objections

objections will be made to these springs on account of there being so many joints; but as there must be either the rub of the spring up and down the back of the pallet, or a double joint: the latter method is certainly preferable to the former.

In Mr. Mudge's watches, the adjusting of his auxiliary spring to prevent tripping was one reason, why they frequently stopped; for when clean, the main spring was adjusted just to raise up the pallets to their detent, and therefore, when the oil got more tenacious, and the works got dirty, the main spring had not power to raise the pallet; the consequence of which was, the watch stopped. Defect in Mudge's.

The pendulum spring is generally allowed by workmen to be the most difficult part of a chronometer to make and adjust well. The two back springs answer the purpose of the regulating power, as well as the maintaining power. Pendulum spring.

As platina is the closest grained metal we have, and it can be drawn very hard, I prefer stopping the holes with it. It burnishes very fine, and oil has no chemical action on it. Preference of platina.

If you think these improvements worthy a place in your Philosophical Journal, by inserting them you will oblige

Your sincere friend,

DAN. DERING MATHEW.

XIV.

Observations on the Possibility of collecting a certain Quantity of Succinic Acid, during the Preparation of Amber Varnish, without any Injury to the Quality of the Varnish: by Mr. PLANCHE, *of the Society of Apothecaries, Paris*.*

HAVING had occasion lately to assist in the fabrication of a large quantity of amber varnish, I remarked, that during the process, and till the heated substance had ac- Succinic acid sublimes in making amber varnish.

* Annales de Chimie, vol. XLIX, p. 40.

quired the proper degree of fluidity, a great deal of succinic acid was given out.

No advantage has been made of this.

Every person, who has made it, must have had the opportunity of observing the same thing; but whether from not knowing the true nature and properties of this salt, or from considering it as essential to the goodness of the varnish, no one, at least that I know of, has thought of turning it to advantage. It would be a mistake however to conclude, that good varnish ought to be free from succinic acid: on the contrary it is very probable, that at the time when the drying oil and oil of turpentine are added, to increase the fluidity of the amber, this substance is still capable of furnishing it, and even in some quantity.

Amber in varnish should not be totally deprived of its acid.

Much of the acid lost in the common way of making.

I should be wandering from the purpose of this notice, if I were to detail the various processes employed for the preparation of this varnish. I shall only say, that, as the process is most commonly conducted on an open fire, and in an open glazed earthen vessel, the mouth of which is four or five inches in diameter, when the matter is sufficiently heated, part of the acid set free is carried off and lost in the air, while a tolerable quantity adheres to the sides of the matrass in the form of very slender needles, sufficiently white to require no purification *.

1½lb. of amber will yield 80 or 90 grs. of acid without detriment.

Every matrass containing 24 oz., which is the common quantity, may furnish 80 or 90 grains of acid, without any injury to the quality of the varnish: a fact of which I have satisfied myself by several trials made in my own laboratory, as well as in that of Mr. Tonnelier, coach painter, who is well skilled in the subject. It is proper to observe here, that we ought to collect the succinic acid as it is sublimed, which takes place a little before the addition of the oxigenized or drying oil. If this operation were deferred, the greater part of it would be lost. In fact, the motion of the spatula necessary to mix the oil with the amber would separate a great deal of the acid: and there is no

Proper time of collecting it.

Acid pure in a new vessel.

Copper best.

* The acid obtained is sufficiently pure when the vessel is new, but it is more coloured in subsequent operations. It may then be purified according to the method indicated by Pott. The artists who use copper matrasses will find an advantage in it, for these vessels being more easily cleaned, they will continue to furnish the same product.

hope

hope of collecting any after the oil of turpentine is added, as this oil, partly converted into vapour by the heat of the mixture, so as to make it swell up or even boil over, occasions the acid to disappear entirely.

However minute the means I have employed for collecting the succinic acid may appear, I think it indispensable to describe them. At first I thought of taking it off with a card. This answered pretty well, but there is danger of burning the fingers, if from inattention they should touch the heated matter. I found a much more convenient instrument was a tin spoon, made as represented in Pl. VI, where fig. 4 is a side view of it, and fig. 5 a front view. This spoon differs from others only in the form of its bowl, which is but little concave, the front of it forming a segment of a circle, and adapted to the size of the matrass; which is represented at fig. 6, but on a much smaller scale, not to occupy too much room in the plate. The bowl of the spoon is terminated behind by a thin plate of iron, which rising a few lines above its edges forms a sort of neck, and to this is joined a handle of the same material, sixteen inches long, forming a right angle with the bowl. The shape of this spoon appeared to me the most convenient, because, 1st, as it adopts itself accurately to the sides of the vessel, it prevents the sublimed acid, which is scraped off by drawing up the spoon, from mixing with the melted amber: and 2dly, it allows the operator to collect it without being incommoded by the vapours emitted.

Method of doing this.

Instrument described.

From what has been said it appears, that artists employed in making amber varnish, without any alteration in their usual processes or apparatus, may furnish us in future with a pretty large quantity of succinic acid, which has hitherto been confined to medical uses, but may soon be found beneficial in other arts. Some trials already give me room to hope, that its solution in alcohol may be employed to imitate the colour of some valuable woods.

Varnish makers may collect a considerable quantity of the acid.

Useful for imitating some fine woods.

XV.

An Essay on the Saccharine Diabetes; by Messrs. Dupuytren *and* Thenard. *Abridged by the Authors.*

Urine changed in diabetes, but its analysis of recent date.

It has long been known, that the nature of the urine is so much changed in the disorder called diabetes, as, instead of being pungent and in small quantity, like that of a healthy person, it is on the contrary saccharine and very copious. The first attempts at its analysis however are not to be dated farther back than thirty years. For this three reasons may be assigned: first the rarity of the disease; secondly the little certainty of the chemical means of analysis formerly employed; and thirdly the common neglect of animal chemistry.

Sugar demonstrated in it.

It was not till 1778 that the existence of sugar in diabetic urine was actually demonstrated. This discovery, made by Caulcy, and confirmed in 1791 by Franck, was conjectured by Willis in the beginning of the 17th century, and in some measure perceived by Poole and Dobson. But it must be confessed, that Caulcy, attending only to the saccharine matter of this sort of urine, left much to be desired. It was necessary to inquire into the other principles it might contain, and particularly those that enter into the composition of healthy urine. This was done in 1803 by Messrs. Nicolas and Quendeville of Caen. From their researches it appears, that diabetic urine contains no sensible portion of uree or of lithic acid; that the most sensible tests scarcely indicate any traces of phosphate or sulphate; that it is impossible to discover in it any free acid; and lastly that we find in it only a large quantity of sugar with more or less common salt.

General state of it in the disorder.

Objects of the authors.

Our object in this essay is not merely to confirm the results we have mentioned, but farther to make known

1. The medical observations we have made on the patient, whose urine we analysed;

* Annales de Chemie, vol. LIX, p. 41.

2, The

2. The very peculiar nature of the saccharine substance we found in this urine: and

3. The various changes this urine underwent before it was brought back to its primitive composition.

PART I. *Observations made on the patient, whose urine we examined.*

Observations on the disease.

From these observations it follows: 1. That the saccharine diabetes may continue several years, and even as long as the digestive powers can maintain themselves, and supply the excessive waste occasioned by the urine.

May continue several years.

2. That this disease is not incurable at any period, not even when the impaired digestion appears unable to supply the materials of the secretion that exhaust the animal economy.

Curable at any period.

3. That the seat of this affection appears to be in the kidneys, not in the intestinal canal.

Its seat the kidneys.

In fact neither the appetite nor thirst of a diabetic patient is any way depraved: they both, as well as the digestive powers, appear merely to be proportional to the want of reparation; in the next place the aliment undergoes the same preparation in the stomach of a diabetic patient as in that of a man in health; and what completely proves, that the digestive faculty is not altered, but simply increased, in diabetic patients, is the quantity of food they take, the quickness with which it is digested, the large proportion of it conveyed into the circulation, and the small quantity of fæces to which it is reduced; and lastly, from the digestion of the food till the secretion of the urine, we find no fluid at all saccharine, or that has undergone any change in its composition.

4. That the cause of the saccharine diabetes appears to be an increased and depraved action of the kidneys; that the saccharine matter of the urine is produced in consequence of this action; and that to it all the symptoms of the disease are to be traced.

Its cause their increased and vitiated action.

5. That the excessive loss, which takes place in this disease, seems under some circumstances to occasion a pretty considerable absorption at the surface of the body.

Superficial absorption increased.

6. That the new proportions established by the saccharine diabetes

Secretions af-

fected as by evacuations in general.

diabetes between the food and the secretions in general, and between their several kinds in particular, are analogous to those occasioned by any evacuation in excess, whatever its nature may be.

Dr. Rollo's treatment infallible.

7. That the mode of treatment recommended by Dr. Rollo, and since so successfully employed by our countrymen, Messrs. Nicolas and Quendeville, and which consists especially in a purely animal diet, is as effectual as the bark in intermittent fevers.

Does not alter the state of the organs.

8. Lastly, that the saccharine diabetes produces no change in the state of the organs, but an exertion of the digestive and urinary organs, both of which are in a state of great activity during this disease, one to prepare and the other to expend the materials of nutrition.

Analysis of the urine.

PART II. *Analysis of the urine of a diabetic patient from the fifteenth day after his admission into the Hôtel Dieu, till he left that place for the hospital of the Medical School.*

Its appearance.

This urine, very remarkable for the largeness of its quantity, emitted a smell, that was not disagreeable. It was limpid, perceptibly yellow, of greater specific gravity than water, and scarcely reddened infusion of litmus. Its taste was slightly saccharine, and at the same time it had something of that of common salt.

Change by keeping.

Left to itself at the temperature of 15° [59° F.], it became turbid in five or six days: bubbles of carbonic acid gas were disengaged on the slightest agitation: the urinous smell it had at first was gone, and it had acquired a smell resembling that of newly made wine: it likewise afforded alcohol by distillation, and became very sour by exposure to the air, so that it exhibited in a slight degree all the marks of a spirituous fermentation.

Distilled.

Distilled in a retort, or evaporated in a capsule, the phenomena it exhibited were the same. It did not become turbid, gradually thickened, and was reduced to a sirup, which sometimes amounted to a seventeenth, sometimes to a twentieth, and never to less than a thirtieth of its weight. From the urine we examined we thus obtained near thirty pounds of sirup, which on cooling always dried into a mass, composed

posed of a multitude of small grains void of consistency. These soft granulous crystals being scarcely sweet, it was natural to suppose, that the substance which formed them was not homogeneal, and included but a very small quantity of the saccharine principle. To ascertain this the following experiments were made.

Saccharine matter distilled.

A hundred parts of this substance were distilled in a retort, the neck of which entered into a receiver kept constantly cool. The products were a great deal of water, but little oil, no ammonia, a larger quantity of gasses that were but slightly fetid, and a tolerably bulky coal, easy to incinerate, and when completely incinerated yielding two parts and half of common salt, and half a part of phosphate of lime.

General conclusions.

From this result we may deduce the following consequences: 1. that this substance contained no animal matter, since it yielded no volatile alkali on calcination: 2. that it contained very little saline matter, since when reduced to ashes it afforded only a residuum equal to a few hundredths of its weight: 3. that it was formed of vegetable principles, since it afforded all their products on distillation.

Fermented with water and yeast.

Presuming sugar to be one of these principles, and not being able to form any conjecture respecting the nature of those with which we considered it to be mixed, we determined to have recourse to fermentation, to destroy the first without altering the others, so that by filtration and evaporation we might obtain them very pure. We put into a large jar 100 gram. [1544·5 grs.] of the substance to be analysed, 25 gr. [386·125 grs.] of yeast, and 500 gr. [7722·5 grs.] of water: to the tubulure of this jar we fitted a tube terminating under a jar filled with water: and the temperature being raised to 18° [64·4° F.], the whole was left to itself. Some hours after these matters had been thus left together, a movement was apparent in some parts of the fluid, which soon became general. A great deal of flocculent matter, from which a considerable number of bubbles issued, was raised up, and carried to some height in the fluid. These bubbles passed rapidly into the jars filled with water, but the flocks fell to the bottom of the vessel, and, giving birth to new bubbles, rose again, to be precipitated

Fermentation active.

Carbonic acid evolved,

alcohol produced,

and a residuum left.

tated as before. This phenomenon, which did not cease for three days, indicated a very active fermentation, and consequently the presence of a large quantity of saccharine principle. In fact near thirteen quarts of pure carbonic acid gas were evolved: the liquor was very spirituous, and contained near 48 parts of alcohol at 40°: and on evaporating to dryness only 23 parts of extract were obtained, formed of 3 parts of seasalt, and 20 parts of a brown viscous matter.

Resembled sugar in its products,

and its habits with reagents.

Now we know, that 100 gr. [1544·5 grs.] of sugar produce 12 gr. [185·34 grs.] of a similar residuum, 56 gr. [864·92 grs.] of alcohol, and 36 gr. [556·02 grs.] of carbonic acid. The substance obtained from diabetic urine therefore gave us by fermentation the same products, and nearly in as large quantity, as the best crystallized pure sugar: and if to this we add, that with nitric acid, alcohol, and other reagents, it comports itself like sugar, we must necessarily consider these two substances as being in some measure identical.

Yet differs from sugar in taste.

Different species of sugar.

We must recollect however, that it is scarcely sweet, and that at any rate it is much less so than sugar. Hence we are led to conclude: 1. that, as chemists have lately begun to imagine, there are different species or varieties of sugar: for here the differences are so striking, that they must convert to a certainty what was only probable. But as the taste is not a certain indication of the existence of the saccharine principle, it became necessary to inquire, whether, among the substances that have hitherto been confounded with sugar on account of their taste, there were not some, that differed from it essentially. We were thus led to examine manna. Our first care was to mix it with yeast and water at the temperature of 18° [64·4° F.], and observe with attention all the phenomena arising from this mixture. The fermentation quickly took place: it was at first brisk, but soon abated: and at the expiration of two days it was at at end. The liquor however had a very strong vinous smell; but, far from being spirituous, it was on the contrary very saccharine; and on evaporation it deposited in the form of crystals almost all the matter that had been employed, divested of the faculty of fermenting.

Manna fermented with yeast and water.

Fermentation brisk, but soon over.

Left a sweet matter incapable of fermenting.

This matter examined.

Though persuaded by these results, that manna contained but

but a very small quantity of sugar, we still deemed it necessary, to compare it with this substance in all its properties, in order to place the fact in the strongest light, and thus discover all the characters proper to the peculiar principle, of which it appears to be almost wholly formed. For this reason we examined the action of alcohol on it, which does not attack the saccharine principle, and that of nitric acid, which does not convert any portion of this principle into mucous acid. The first of these reagents, at the temperature of 60° [140° F.] dissolved so large a quantity of manna, that on cooling it formed a mass of crystals in groups, the crystals in each group issuing from a common centre. The second produced in it by long continued boiling such a large deposit of mucous acid, that the weight was nearly equal to half that of the manna employed.

Hot alcohol took up so much as to become solid on cooling.

Nitric acid converted half to mucous acid.

Here then we have two more characters, that strikingly distinguish sugar properly so called from the peculiar principle of manna.

These characters distinguish manna from sugar,

No doubt farther research would exhibit many others more or less striking; but as those we have related are sufficient, to make those two substances be considered as perfectly distinct from each other, we did not think it necessary to push our examination farther.

and perhaps others.

Hence it follows, that it will always be an easy matter to discover and to separate manna, or rather the peculiar principle of manna, whatever be the substances with which it is mingled. All that is necessary is to treat the matter containing it with hot alcohol, and it will be almost entirely precipitated by cooling. Indeed there are other vegetable substances, that possess this property even in a striking degree; but as these substances are found only in this class of acids, it is always practicable to deprive it of these, by combining it with an alkaline or earthy salifiable base, or a metallic oxide, according to the nature of the acid; and consequently this mode of separation may be generally employed.

This principle of manna separable by alcohol.

Thus we may ascertain, whether the honeydew observed on the leaves of certain trees, particularly those of the lime, be really a species of manna; and if it be the same with the saccharine principle that exists in asparagus, and which

Honeydew?

and saccharine principle in asparagus?

Messrs.

Messrs. Vauquelin and Robiquet have found there mixed with a peculiar principle.

PART III. *Analysis of the urine of the diabetic patient, from the time of his admission into the hospital of the Medical School till he quitted it.*

Medicine without regimen had no effect on the urine.

During the time the patient was in the Hôtel-Dieu, he could not be confined to any regimen. He lived nearly as he pleased; his disorder remained stationary, and his urine, which was still very abundant, had not altered its nature. It was then determined, to remove him to the hospital of the Medical School, where, being almost always under the eye of Mr. Dupuytren, who had the care of him, or of some one of his pupils, it was much more easy, to oblige him to do whatever was desired.

Vegetable food being withheld

At the expiration of a few days all kinds of vegetables were refused him, and nothing was given him but animal food. The quantity of this he took, as well as of what he drank to satisfy an unquenchable thirst, was accurately weighed.

in a few days a change took place.

For the first three or four days no change in the urine was observed; but in five or six it was less white, more acrid, more acid, and less saccharine. Subjected to evaporation, instead of remaining limpid as before, it became turbid, and was covered with a tolerably thick pellicle of albuminous matter. When I perceived this change, particularly the presence of animal matter in his urine, though the state of the patient was completely unknown to me, and I was unacquainted with the manner in which he had been treated, I concluded, that the disorder had begun to abate: and then finding, that this animal matter became daily more abundant, I considered the cure as approaching. Mentioning my opinion to Mr. Dupuytren, he said it was probable, but appeared surprised at it, till I informed him on what it was founded.

Albumen appeared in it,

and increased in quantity.

The albumen diminished, & uree and lithic acid began to appear.

From that time the patient continued to amend. His urine grew daily more animalized, and less saccharine. The albuminous animal matter soon began to diminish gradually, and the uree and lithic acid as gradually reappeared. At length it became perfectly similar to that of a man

man in health, and the patient was cured. Immediately on this however he indulged in excesses of various kinds, when the diabetes returned, complicated with other disorders, under which he soon sunk.

Excesses brought on a fatal relapse.

If now we take a review of all the inductions that may be made from the experiments just related in the second and third part of our memoir, we may affirm

General conclusions.

1. That the diabetic urine we examined was composed almost wholly of a substance but little saccharine: and that nevertheless it possesses all the properties that characterize sugar; for it is converted into alcohol and carbonic acid by fermentation, affords a great deal of oxalic acid and no mucous acid when treated with nitric acid, is very little soluble in alcohol at 36°, and produces when calcined but little oil, and a great deal of water and carbonic acid. And thus it is demonstrated, that there are different varieties of sugar.

State of the urine.

2. That manna is not a species of sugar: that it contains but a small quantity, which may be destroyed by fermentation: and that, on the contrary, it contains a great deal of a peculiar principle, the taste of which is very sweet, and the chief characteristics of which are not to ferment with yeast, to yield a great deal of mucous acid with nitric acid, and to be more soluble in hot than cold water, but particularly in alcohol, so that the solution on cooling becomes a crystalline mass.

Manna not sugar.

3. That if nothing but animalized food be given to diabetic patients, their urine changes its nature pretty quickly: that at first we find in it an albuminous matter; that this albuminous matter the quantity of which continues increasing for some days, appears to be an unequivocal sign of a cure: that afterward the albumen gradually disappears: that the kidneys then begin to secrete uree, lithic acid, and no doubt acetous acid also: and that the urine soon becomes similar to that of a person in health: but that nevertheless, to prevent a relapse, the patient ought still to continue his regimen of an animal diet for a considerable time, and take nothing that might bring on the diabetes afresh.

An animal diet will cure,

but must be continued long to prevent relapse.

XVI.

XVI.

Letter from Mr. ROLOFF, *of Magdebourgh, on the fetid Resin of Sulphur**.

Fetid resin of sulphur

I lately had an opportunity of detecting Mr. Westrumb's fetid resin of sulphur † in an unexpected manner.

obtained in making golden sulphur of antimony.

Mr. Michaelis, after having precipitated the golden sulphur of antimony from the hidroguretted sulphuret of antimoniated potash by means of sulphuric acid, evaporated the supernatant liquor, which held the sulphate of potash in solution.

Heated smells like burning asafœtida

When the solution began to be concentrated, a vapour arose, by which the artist who was stirring it was singularly incommoded. At the same time an insufferable stench was emitted, resembling that of burning asafœtida.

The saline mass, being evaporated to dryness, was of a gray colour, and had the remarkable smell just mentioned.

Gives to alcohol the taste & smell of garlic.

Being digested with alcohol, it imparted to it the smell and taste of garlic.

The alcoholic solution, left to evaporate spontaneously, yielded a gray, glutinous mass, having a similar taste and smell.

Easily procurable in quantity.

I was desirous to impart the knowledge of this fact, as I know not whether Mr. Westrumb be acquainted with the formation of a large quantity of the fetid resin, which may easily be procured by this process.

Formed independently of alcohol.

Since the smell displays itself before any alcohol is added, we may conclude with Mr. Westrumb, that the alcohol does not contribute to its formation.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Society.

Pinna ingens.

Geognosy of Inch-Keith.

At the last meeting of the Wernerian Natural History Society, June the 11th, Dr. Thomas Thompson, one of the Vice-Presidents, read a very interesting and valuable paper on the chemical nature of fluor-spar. Captain Laskey also read a paper on the pinna ingens of Pennant: from his observations, it appears, that the pinna ingens of Montague, pinna corealis of Stewart, and pinna ingens of the Linnean Transactions, are the same species, and identical with the pinna ingens of Pennant. At the same meeting, Charles Anderson, Esq., read some observations on the geognosy of the island of Inch Keith, in the Frith of Forth. It appears from the interesting details which he communicated, that the whole island is composed of rocks belonging to the independent coal formation; and that the greenstone, which there occurs, is traversed by true veins filled with quartz, chalcedony, calcspar, &c, and also contains numerous contemporaneous veins of different kinds. Mr. Anderson intimated his intention of laying before the Society, at a future meeting, a more particular description of the island, illustrated by drawings and a series of specimens.

* Extracted from Gehlen's new Chemical Journal. *Annales de Chimie*, vol. LXII, p. 190.

† See Journal, vol. XVIII, p. 41.

TWO

TWO METEOROLOGICAL TABLES for 1807,

Communicated by Dr. CLARKE, of Nottingham.

QUANTITY OF RAIN,

WHICH FELL AT THE FOLLOWING PLACES IN THE YEAR 1807,

In Inches and Decimals.

By the REV. J. BLANCHARD, NOTTINGHAM, who solicits Communications.

1807.	*Chichester.*	*London.*	*Diss, Norfolk.*	*Chatsworth, Derbyshire.*	*Horncastle, Lincolnshire.*	*Ferriby, Kingston-upon-Hull.*	*Heath, near Wakefield, Yks.*	*Lancaster.*	*Dalton, Lancashire.*	*Kendal.*	*Sedberg, Yorkshire.*	*Nottingham.*
JANUARY,..	2·41	0·64	1·37	1·40	1·50	0·82	0·85	3·33	2·75	2·92	2·38	0·75
FEBRUARY,	2·44	1·48	1·66	1·79	2·77	2·64	2·09	3·59	4·59	5·58	4·00	1·23
MARCH, ...	0·23	0·50	1·36	0·44	1·69	1·21	2·60	1·12	1·52	2·21	0·57	0·73
APRIL, ...	0·00	1·02	0·81	0·67	2·36	1·77	1·17	3·19	3·66	2·90	1·72	0·94
MAY, ...	5·47	3·26	3·47	5·26	2·80	2·53	4·70	3·75	3·97	4·47	2·86	5·08
JUNE, ...	0·56	1·74	1·92	2·81	2·52	1·13	2·65	1·25	2·26	2·27	4·00	3·00
JULY, ...	1·62	0·38	1·54	2·26	2·25	1·11	2·43	3·50	3·74	4·48	3·43	2·55
AUGUST, ...	3·13	1·94	1·64	1·57	1·27	3·31	2·18		2·92	3·49	4·58	1·40
SEPTEMBER	3·22	2·18	2·17	1·27	1·45	2·86	3·34	10·08	10·27	7·92	6·86	1·70
OCTOBER,..	2·48	0·94	0·90	3·13	1·78	1·93	1·60		6·08	7·09	5·15	1·70
NOVEMBER	7·54	3·36	2·27	1·18	3·83	6·02	5·57	4·00	4·93	5·07	5·50	3·33
DECEMBER	0·83	0·76	1·06	2·67	0·91	1·62	0·86	3·20	3·26	4·53	2·64	0·93
Total	29·93	18·20	20·17	24·45	25·13	26·95	30·04	37.01	49·93	52·93	43·69	23·32

A Meteorological Table, from June to December, 1807,

By DR. CLARKE, of NOTTINGHAM.

☞ The following observations on the Thermometer are made at 8 A. M., 2 P. M., and 11 P. M.; and on the Barometer at 2 P. M. The former instrument is placed in the open air, exposed to the west, but in a situation surrounded by buildings, which prevent any alteration of temperature from currents of air. The direction of the Wind is taken from the vane of St. Peter's Church; and the numbers state how often it has been observed in any particular quarter during the month.

1807.	THERMOMETER.				BAROMETER.				WEA.		WINDS.			
	Highest.	*Lowest.*	*Mean.*	*Greatest variation in 24 hours.*	*Highest.*	*Lowest.*	*Mean.*	*Greatest variation in 24 hours.*	*Fine, or Fair.*	*Snowy, or Rainy.*	*N. and N E.*	*E. and S E.*	*S. and S W.*	*W. and N W.*
JUNE, ...	75°	46°	57°·85	10°	30·31	29·53	29·95	·33	20	10	24	5	24	37
JULY, ...	80	52	64·00	8	30·50	29·52	29·90	·56	17	14	11	8	55	19
AUGUST, ...	78	53	64·98	9	30·18	29·59	29·85	·34	28	3	11	11	53	18
SEPTEMBER..	67	40	51·93	10	30·15	29·21	29·69	·55	15	15	5	1	45	41
OCTOBER,....	65	40	53·29	14	30·15	29·19	29·83	·51	23	8	8	8	51	26
NOVEMBER,..	50	26	38·93	11	30·10	28·43	29·44	·80	14	16	16	2	41	31
DECEMBER,..	50	24	38·14	13	30·24	29·11	29·84	·55	25	6	2	9	48	34
Avr. for 7 Months	—	—	52°·73	—	——	——	29·78	Totl.	142	72	77	44	315	206

METEOROLOGICAL JOURNAL

For JUNE, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

N. B. For want of room in the present number, the apparatus and its relative situations will be described in our next.

MAY. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
30	64	63	70	56	30,30	Fair	Fair
31	69	64	74	55	31,30	Rain	Ditto
JUNE.							
1	60	56	64	49	29,76	Fair	Rain
2	62	58	66	48	30,4	Ditto	Fair
3	63	60	67	57	29,79	Rain	Ditto
4	61	56	63	52	29,71	Fair	Rain
5	62	56	64	51	29,68	Ditto	Fair
6	53	52	57	49	29,72	Rain	Rain
7	58	56	62	51	29,84	Fair	Ditto
8	60	55	63	51	29.82	Rain	Fair
9	54	55	62	52	29,66	Cloudy	Rain
10	58	56	64	49	29,87	Fair	Fair
11	63	58	67	53	30,4	Ditto	Rain
12	60	59	69	52	30,18	Ditto	Fair
13	66	63	70	55	30,15	Ditto	Ditto
14	67	63	70	55	30,7	Rain	Ditto
15	64	62	68	50	29,92	Fair	Ditto
16	62	61	65	56	30,12	Ditto	Ditto
17	64	62	70	59	30,14	Cloudy	Ditto
18	67	68	74	62	30,6	Fair	Ditto
19	68	67	74	62	30,11	Ditto	Ditto
20	67	66	71	60	30,6	Ditto	Ditto
21	67	62	70	58	30,—	Cloudy	Ditto
22	69	62	72	56	29.83	Rain	Ditto
23	62	58	68	54	29,72	Fair	Rain
24	63	62	68	55	29,89	Ditto	Fair
25	63	62	69	56	30,4	Ditto	Rain
26	63	61	69	55	30,6	Ditto	Fair
27	61	60	62	54	30,6	Cloudy	Ditto
28	58	58	62	54	30,9	Fair	Ditto

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1808.

ARTICLE I.

Observations on the crystallized Substances included in Lavas: by G. A. DELUC.

(*Concluded from p.* 188.)

"EVERY thing in volcanoes indicates, that the depth of their foci is immense." These are the words of Mr. Fleuriau de Bellevue, and he adds, "This is the opinion of Mr. Deluc and several naturalists."

Foci of volcanoes very deep,

I have said, and I believe, that the foci of the volcanoes are at very great depths, contrary to the opinion of those naturalists, who imagine the foci to be very near the base of the volcano, and even place them in the cone, that rises above the ground: an opinion so repugnant to all the phenomena, that I cannot conceive how it could enter into any one's head. I do not think however, that I have used the word *immense*, which would imply a depth below the reach of conjecture, and this is far from my idea. A league perpendicular is a very great depth, and I do not suppose the foci of volcanoes can be much deeper; but every thing indicates, that they have ramifications. The fragments of natural rock they throw out can come only from these late-

but not of an immense depth.

Ramify among the strata.

 ral

ral galleries, from which they are broken off and carried along by the lava that flows through them. Another phenomenon indicates the same thing: this is the burning places that manifest themselves at the bottom of the sea in the environs of a volcano during an eruption, and which are at the same time a sign, that the focus is not at a depth to be called *immense*. I particularly remark this expression, because from this presumed depth have been deduced theories respecting the formation of the globe, that are destitute of foundation.

Some deeper than others.

What in fact is the depth that may be inferred from volcanic phenomena, compared with the diameter and solidity of the globe? This depth is no doubt more or less, according to the mass raised up by the volcano. Thus it is probable, that the foci of Etna, the peak of Teneriffe, and the volcanoes of Peru, are deeper than those of Vesuvius, Vulcano, and Stromboli. This is all we can conclude; and nothing respecting the origin of our globe, or the events that have concurred in its formation.

We alter the form of minerals,

but cannot restore them to their primitive state.

" Man separates, dissolves, brings together, and combines minerals, and causes them to change their form." All this is true: he does it by his solvents, and the fire of his furnaces; but it is not added, that there is no method, no fire whatever, by which he can reſtore them to their state of mineralization. He is no more capable of doing this, than of regenerating the plants he has burned and reduced to ashes. We are very far indeed from being able to produce any thing similar to the rocks, the crystals, the minerals of our mountains. This single reflection overturns every system, that ascribes the formation of these substances to fire, since all the operations of natural and artificial fire that we know, and we can reason only from these, produce nothing similar to them.

We should be cautious therefore in comparing our means with those of nature.

These limits, which human means cannot pass, should render us very circumspect concerning the rseults ascribed to them, since no one of the natural substances, that man destroys or alters the nature of, can reappear again, but by following the laws and order established by the Creator from the origin of all things.

Limestone

Mr. Fl. de Bellevue mentions a *singular production* of a lime-

lime-kiln, which he quotes as an example in favour of his system. "This production," says he, "resembles internally certain hornblendes of the Alps, and compact and homogeneous lavas. Its external part is puffed out like that of lavas, its surface is covered with a yellow glaze, and its cavities are lined with little crystals." In a note he adds: "let not the reader suppose, that the stones of this kind had fallen accidentally into the kiln, as this was impossible."

supposed to be converted into hornblende or lava in a kiln.

I shall offer no direct objection to the fact; as this would require a knowledge of the production itself, and particularly of the vicinity of the lime-kiln: but I shall offer a general remark, that may throw some light on its origin. It is a very common circumstance, for fragments of other stones, which the workmen have overlooked, to get among the broken limestone, with which the kiln is filled, and not to be observed till the lime is taken out. In this case, instead of a piece of lime we find a stone covered on its surface with a vitreous glaze, which being broken appears to be granite, serpentine, or some other vitrifiable stone. Instances of this are frequent in the lime-kilns in my neighbourhood. To be certain, that such fragments could not be introduced, there must be nothing but calcareous rocks in the country, these must even be free from quartzose or siliceous nodules, and there must be no other kind of stone either belonging to the soil or adventitious. Thus it is very probable, and from a great number of instances I am persuaded it was the fact, that the product of the lime-kiln abovementioned was originally a stone of a different kind from that commonly burned in the kiln.

But pieces of other stone get into limekilns accidentally.

"Naturalists," continues Mr. Fl. de Bellevue, "who still believe, that rocks on which volcanic fires have acted have experienced only an imperfect fusion, and that their crystals have remained intact amid their fluid paste, are obliged to have recourse to a multitude of suppositions, to explain the state in which the lavas are found when cold."

That lavas are incompletely fluid said to be a supposition.

These naturalists have recourse to no supposition: they find it not necessary. Nothing in the lava changes its form or nature when it cools. The foreign substances it contains

But it is a fact.

in its incandescent paste retain their form: no change takes place, the fire of volcanoes not being sufficiently intense, to fuse them or alter their nature. I have adduced a great many instances of this.

Volcanic fires could not fuse solid rocks.

On this occasion I shall recal to the reader's mind the idea I suggested respecting the probable state of the subterranean strata, from which the lavas issue. We see, that to reduce stones or minerals to a state of fusion, they must be broken into very small pieces: but there are neither pestles nor stampers in the strata from which the lava originate; and volcanic fires are as incapable as those of our furnaces, to fuse rocks in a solid mass. These strata then must be in a pulverulent and muddy state, to be capable of being fused. In such a state we can easily conceive chemical affinities may exert themselves, and form crystals either solitary or in groupes, that would remain enveloped in the matter in fusion. How is this fusion effected? whence arise the fires that occasion it? We perceive from its emanations, that sulphur is the principal ingredient, that iron enters into the mixture, and that muriatic acid and sal ammoniac likewise form a part of it. But what circumstance, what combination is necessary, to excite the fermentations, that produce the fires, the fusion, and all the phenomena of volcanoes? On this we shall never be able to do more than form conjectures, some of which may approximate to the truth, and others be very wide of it. But as no means we are capable of employing can prevent any of them, it is of little importance, whether our conjectures on the origin of these fires, and the manner in which they act, be just or erroneous. All that is essential is not to ascribe to them a greater extent, more activity, and a wider influence, than they really have; that we may not be led to form systems on mistakes or exaggerations.

In these fires sulphur is the principal ingredient, with iron, muriatic acid, and ammonia.

Sea-water said not to be necessary to volcanic fires,

as an eruption in Mexico 100 miles from the sea.

Mr. Fl. de Bellevue does not admit, that sea-water is absolutely necessary to produce volcanoes; and he quotes in opposition to this opinion, which at first appeared to him very plausible, a volcanic eruption mentioned by Messrs. von Humboldt and Bonpland, which took place in 1759, "in a plain in Mexico, forty leagues from the sea in a direct line; an eruption that in one night threw up a volcano of

of 1494 feet [1592 Eng.] high, surrounded by more than two thousand mouths, which are still smoking."

If burning volcanoes could manifest themselves any where, without being within reach of the influence of the sea, we should not have a single instance of the kind quoted, for numbers would exist: and if this had been the case, I should not even have thought of the opinion I have advanced. But after having attended to this general fact, that there is no burning volcano in an inland country, and that no extent of fresh water, however large, has produced one; all being near the sea, or surrounded by its waters: and having observed, that the vapours of volcanoes deposit abundance of muriatic acid: I hence deduced this indisputable inference, that sea water is absolutely necessary, by the salts it holds in solution, to produce the fermentations that raise and feed volcanoes. But the contrary appears from the situation & circumstances of volcanoes.

This conclusion has since been confirmed by the eruptions of water from the volcanoes in Iceland, which deposited common salt in large quantity; and lately by an observation of Messrs. von Humboldt and Buch, who were witnesses of the eruption of Vesuvius in August 1805, and perceived the sides of a cleft in its crater lined with a crust of sea salt two or three inches thick. Sea-water thrown up by Hecla. Clefts in Vesuvius encrusted with salt.

Hence it follows, that the fact quoted by Mr. Fl. de Bellevue proves nothing more, than that there may be subterranean channels extending forty leagues from the sea, and that on some occasion its waters penetrated into them; or perhaps their influence was merely extended gradually to that distance. It is even very probable, that, if all the circumstances accompanying this fact were fully known, a more precise explanation of it might be given. In 1538 an equally sudden eruption raised up the *Monte-nuovo* near Naples. The Mexican volcano accounted for.

"All those who have seen volcanoes in a state of activity," says Mr. Fl. de Bellevue, "assert, that nothing is equal to the violence and immensity of their fires; and yet some appear without hesitation, to rate the power of volcanoes even below that of our paltry furnaces." Fires of volcanoes said to be violent and immense.

A volcano during an eruption exhibits such a grand and awful sight, that it lays hold of the spectator's imagination, But this imaginary.

and

and throws him into amazement. This is the effect of the extent of its fires, the noise with which they are accompanied, and the sight of the streams of burning lava. But if he examine it with inquisitive attention, he will soon judge from its effects, that this vast furnace has in no part a heat so intense as may be produced in our iron works; and it is easy to perceive, why our furnaces have this greater intensity of heat: it is produced by the continual current of air blown into them, which by its extreme rapidity is incessantly bringing fresh air, the presence of which imparts greater activity to the fire; but this cannot happen to the fire of a volcano, which has no such communication with the air. This is the reason why the pyroxene schoerl, which is unalterable by the fire of volcanoes, is reduced to the state of glass in a crucible in our iron furnaces, and fragments of lava exposed to a similar trial are more completely vitrified.

The heat inferior to that of our furnaces; and must be for want of a current of air.

Obsidian a perfect glass.

Of all volcanic substances the obsidian, or compact glass of volcanoes, has been exposed to the greatest heat. The vitrification of this is complete. None of the pieces I possess, or have seen, exhibits any thing but glass. All the substances that compose it have been reduced to perfect fusion. These vitrifications therefore come from a part of the focus, where the heat has been urged to a higher degree by some particular circumstance.

And this includes no crystals.

Now why is it that these obsidians, which must have cooled as slowly as other lavas, do not exhibit any crystalline figure within them; if it be not for this reason, that, all the substances in them having been fused, there can be nothing but glass throughout their mass?

We must not reason from our operations to nature's.

I will say in my turn, that it is much more extraordinary for men to set out from the operations of our petty manufactories, to determine the force of the fires of volcanoes, and assign them an unlimited extent; and still more extraordinary thence to deduce the origin and formation of rocks and primordial mountains. Let us confine ourselves to the effects, that our narrow means can produce; and not plunge ourselves into a labyrinth of illusions, by reasoning from small to great; for, as our means are merely artificial, they are not those that operate in nature.

"Naturalists,"

"Naturalists," says Mr. Fl. de Bellevue, "who imagine, that the crystals contained in lava have remained intact amid their fluid lava, pass by without notice the observation of those, who, as Spallanzani and Hubert relate, have seen the lava spout up at different times like water issuing from a fountain, form a number of very brisk streams, and possess a degree of fluidity sufficient to insinuate itself into the smallest interstices of the bodies it penetrates:" and he adds in a note: "Mr. Faujas has in his collection a piece of a palm-tree from the Isle of Bourbon, which proves, that the fluidity of the lava has been very great, since it has insinuated itself into the very fibres of the wood."

Lava said to be as fluid as water.

Piece of palm, into the fibres of which lava has penetrated.

This fact, if it were real, would prove an impossibility, namely, that lava might be in a state of fusion without being red hot: for a substance as combustible as a piece of a palm-tree, or any other vegetable, would have been burned and consumed, or reduced to a coal at the first contact of the lava. It must be an illusion therefore; and either the substance surrounding the palm is not lava, or the matter surrounded is not a vegetable substance. This illusion, strange as it is, is not single in its kind. In an account of a tour to Iceland, translated and published at Paris in 1802, I have read, that the Danish travellers imagined they discerned wood in a piece of lava of Hecla. Count Borch made the same mistake, and even greater, for he says he saw "pieces of wood slightly scorched" in whole rocks of the lava of Etna.

This an impossibility.

Others have made the same mistake.

I have in my possession a large piece of vitreous lava, that I brought from the island of Vulcano, which may serve to explain this illusion. It has very large blebs, which are drawn out considerably in length by the flowing of the lava, and their surface is streaked with threads, which have the appearance of woody fibres; and this appearance is heightened by the tint they have derived from the vapours, that are continually exhaling from the matter in fusion. Several persons, who have seen this fragment, have taken it at first view for wood. I have another piece of vitreous matter from Lipari, which is drawn out into such fine and close threads, that no fossile agatized wood, let its fibres be ever

Vitreous lavas sometimes closely resemble fossil wood.

so

so distinct, would have a stronger resemblance of wood than this, were it not for its glassy lustre. I have another piece, vitreous likewise, one of the surfaces of which, that was exterior, is marked with a multitude of very small threads, arranged in some places in undulations resembling the woody fibres round a knot.

This the case with the supposed piece of palm.

From these examples I am led to believe, that the specimen from the Isle of Bourbon is wholly lava, with a woody appearance on one of its faces; for at all events a vegetable, even in the state of wood, could leave nothing after its combustion, which must be inevitable, but a vacuity in the lava, and traces of charcoal; and by no means the impression of woody fibres, still less the fibres themselves.

In support of his principal opinion Mr. Fl. de Bellevue adduces several arguments, which I shall pass over, because our business is with facts, not conjectures. One of them is the following. "The great volumes of lava, that act the principal part in volcanic eruptions, burst out from the crater, from the sides of the mountain, or from its base. They proceed with rapidity from the very focus of the volcano, possessing an incomparably greater degree of heat than the matter that rests in the crater. This heat, this rapidity, cause them to spout forth and flow like water, and cannot permit any crystals to form in them. All that are found in it afterward are produced during its repose and refrigeration."

Grand eruptions flow like water, therefore the crystals formed in it while cooling.

I must first remark, that these expressions frequently repeated, that lava spouts out and flows like water, are merely metaphorical; for lava, far from flowing like water under any circumstances, leaves in succession, by hardening, all its matter on the ground it flows over.

Flowing like water a metaphorical expression.

Mr. Fl. de Bellevue did not recollect the lava of Etna of 1669, which I have already mentioned. This lava, issuing from the base of that great volcano, traversed a space of ten miles in length, and advanced into the sea, where it accumulated in prodigious heaps, after having covered its route with its matters to a vast extent in breadth and thickness. This certainly was to be reckoned among the lavas, that act the principal part in volcanic eruptions. Now this lava, of which I have pieces before my eyes, I again assert

Eruption of Etna of 1669.

is

is filled throughout its whole extent, from the extremity of its destructive course to the place where it issued from the crater, with a multitude of pyroxene schoerls, of those whitish crystalline laminæ I have described, and of small chrysolites; and the crater, from which it issued, threw up myriads of the same substances. Can we discern here formations produced at the time of the cooling of this lava, since all these crystals existed there at the moment of its greatest fusion and heat, the focus of the eruption itself having thrown up loose ones from its crater in multitudes innumerable?

The naturalists who have remarked, that leucites and pyroxene schoerls are crystals not to be found in the strata coming under our observation; and who have hence inferred, that they would have remained for ever unknown to us, if volcanic eruptions had not brought them to light; are certainly well founded in their opinion. Mr. Fl. de Bellevue however terms it a supposition. But nothing is more true than the observation, and nothing can be more natural than the consequences deduced from it.

Leucites and pyroxenes not found in the strata we have penetrated, but come from others beneath.

"We have seen," continues he, "that there is no example to prove, that aqueous solutions now form, or are capable of forming, rocks similar to the primitive ones; and that fire on the contrary daily exhibits to us productions, that are not simply analogous, but even identical with them."

Rocks of the primitive kind not now formed in water:

On the contrary we have seen, that the productions of fire have only an apparent, not a real resemblance to primitive, or, to speak with more accuracy, primordial rocks. The fires of volcanoes, like those of our furnaces, have not produced and never will produce any thing like them, because these primordial strata do not owe their origin to fire.

Neither will aqueous solutions form such rocks; for they were produced by precipitation from the primordial fluid at periods not remote from the origin of the globe, and every thing indicates, that they no longer continue to be formed. The water of the present sea does not now contain the requisite elements, for of these it has been deprived. The mud of rivers, of which some imagine they may

because their principles, which existed in the primitive ocean, are no longer found in the sea.

The deposi-

tions of rivers cannot form them, may be formed, but which cannot form them, does not reach the bottom of the sea: its waves drive it back, and keep it on the shore, where it adds daily to the first boundaries of the continents.

and are too small to affect the level of the ocean.

On this occasion I shall repeat a remark I have several times made. These additions are so trifling, compared with the extent of the sea, that they cannot produce any perceptible change in its level. It is these additions to the land that have been so often mistaken, and quoted as proofs of the retiring of the sea.

Volcanoes could not be distinguished from other mountains, if they did not differ from them.

By what signs can we know ancient volcanoes wherever they exist remote from the sea, if not by their form, and the nature of the substances that distinguish them? They must then be different from all other mountains, or they would be confounded together, and these could not be distinguished from volcanoes. The truth is then, that all the mountains we know, the Alps, the Jura, the Pyrenees, and all those of our continents, have no relation to volcanic mountains; that their strata, and the matters that compose them, have been formed in water, and fire has had no concern in their production.

Valley of Quito

It was from these distinguishing and invariable characters of volcanoes, and of the soil around them, that in my preceding observations I employed the following expressions. "When the valley of Quito, and the mountains that border it, shall be observed by naturalists experienced in the knowledge of volcanoes and volcanic substances, I have no doubt they will perceive, that the state of things is as I have said." I should have been far from thus expressing myself, if other lands and other mountains had been the subject. But the great mountains that skirt that celebrated valley on either hand being certainly volcanoes, three of which are not yet extinct, and its soil being composed of their vast eruptions, I could venture to give this opinion without apprehension of going too far, or of wanting that proper diffidence a man ought to have in his own knowledge.

and the volcanoes bordering it.

Ancient volcanoes fewer than some assert.

The ancient volcanoes observed on the surfaces of continents are not so numerous as Mr. Fl. de Bellevue imagines, when he says, that volcanoes, either burning or extinct, are seen every where on the face of the globe. This is a great

great exaggeration. Many are seen in various places, no doubt; but the space they occupy bears no comparison with that where there are none. I include ancient and extinct volcanoes, for those still burning are very few. There are only four in Europe: those of Iceland are in a distant latitude.

Burning ones very rare.

This reminds me of a similar opinion of Mr. Patrin, which he gave of Italy. It is in his *Recherches sur les Volcans*, "Inquiry concerning Volcanoes according to the Principles of the pneumatic Chemistry." He says, "Italy is full of volcanoes, and covered from one end to the other with lavas and tufas of enormous thickness." Yet the true fact with respect to Italy is, that the Apennines, which traverse it from one extremity to the other, all the ramifications of that chain, and all the eastern shore of that peninsula, have nothing volcanic in them; and that the soil of this kind lies only on the western coast, where it is frequently interrupted by aqueous strata.

Patrin's misrepresentation of Italy in this respect.

When explanations of the manner, in which a fact in terrestrial physics, that is in some degree obscure, may have happened, deviate from the most natural, and that which is most conformable to all the phenomena, they may be very different from each other, or even opposite. Thus it happens, that the naturalist I have just quoted, being equally of opinion, that the pyroxene schoerls did not preexist in the lava, separates them from the matter of the lava, and makes them arise "from an aeriform fluid, which has passed to a solid consistence by the effect of attraction." This question I have already discussed with precision, and to some extent, founding all I have said on facts, in my Observations on Pyroxenes, or volcanic Schoerls, in the *Journal de Physique* for March, 1801.

When people deviate from nature they often support the same opinion by contradictory arguments.

From all the facts I have adduced the following conclusions may be considered as established.

General conclusions.

That every volcano, whether burning, extinct, or ancient, whatever its height or extent, and wherever situate, is a mountain of a class distinct from all others: that it is formed by no *neptunian* strata: that all the solid substances constituting it are the products of fire: that it has been raised up, from its base to its summit, by the accumulation of

Volcanic mountains formed by fire, and different from all others.

of matters successively thrown up by its eruptions, the focus of which is beneath all the strata with which we are acquainted.

Crystals in lava foreign to it.

That the crystalline substances included in lava are foreign to it: that they have been formed anteriorly in the humid way in strata, which the volcanic fires have reduced to fusion leaving their crystals untouched, because those fires had not sufficient intensity to fuse them.

The whole mountain is a volcano.

That we should cease to say volcanoes manifest themselves on the summits of mountains, because volcanic mountains entire constitute volcanoes. This is the reason why new mouths frequently open in their sides, or at their base.

Sea water necessary to it.

That sea water is absolutely necessary, by the salts it holds in solution, to excite the fermentations that produce volcanoes.

All other mountains and hills produced by water.

That all the strata and substances, which compose calcareous, schistous, or granitic mountains, and all their varieties, as well as sandy, gypseous, and argillaceous hills, are the work of water.

All ancient volcanoes have burned under the sea.

That all the ancient volcanoes, which are now inland, have burned underneath the waters of the sea. The schists and granites which appear around some of them are foreign to them, belonging to strata through which the eruption forced a passage, and which have remained bare. They would have been buried under the volcanic matter, to be seen no more, if those volcanoes had been longer active. Those which were burning at the time when the sea retired from our continents ceased to burn at that period: a period beyond the memory of the inhabitants of the country, because there could be no inhabitants of the land round those volcanoes, when it formed part of the bottom of the sea.

Volcanic sand between two calcareous strata.

Among the numerous facts that prove this truth, count Marzari of Vicenza has furnished me with a very remarkable one, on his return from a tour in Auvergne. At Santourgue there is a stratum of volcanic sand, about six inches thick, between two calcareous strata. After a calcareous deposition had been formed therefore on the sides or base of the volcano, an eruption must have thrown out and spread

spread this sand, upon which a fresh calcareous deposition took place; and these operations could have occurred only in the sea. Count Marzari has had the goodness to present me a specimen of this sand, which is similar to what was thrown out of the superior aperture of Etna in the eruption of 1763, which I have mentioned above.

I shall remind the reader here of what I have said several times, that to distinguish the different periods when volcanoes have been burning, and not to confound them together, it is proper, to call those *ancient*, which have burned in the sea before our continents were laid dry; and those only *extinct*, which by their situation are still capable of burning, if the inflammable matters that gave rise to them were not consumed. But this necessary distinction will never be made, as long as it is believed, in spite of the dictates of observation and experience, that burning volcanoes may exist independent of the waters of the sea. Distinction between ancient and extinct volcanoes.

Mr. Fl. de Bellevue must be convinced, that my sole object, in making these observations, is to illustrate in a more precise manner the grand phenomenon of volcanoes, that we may not ascribe to them effects in which they have no concern, or deny them those they have really produced. These limits, grounded on well established facts, can alone free us from systems founded on contrary notions, and afford more certain bases to geology, that interesting and important branch of terrestrial physics. The author's object.

II.

Experiments on Molybdena. By CHRISTIAN FREDERIC BUCHOLZ.

(Concluded from p. 196.)

VIII. *Manner in which molybdena comports itself with certain acids.*

Exp. 29. TEN grains of powdered molybdena were put into half a drachm of sulphuric acid of the specific gravity of With sulphuric acid.

of 1·86, and left for twenty-four hours at an ordinary temperature. The acid did not exert the slightest action on the metal. At a moderate heat a large quantity of sulphurous acid was evolved, the liquor became of a yellowish brown colour, and it assumed a sirupy consistence. It was then diluted with four times as much water, and became of a brownish yellow. After standing some time a little molybdena was deposited, which had not been dissolved. The liquor having remained some hours in contact with the metal, it gradually turned green, and afterward blue: but the most remarkable circumstance was, that part of the blue oxide precipitated itself in the form of a very fine powder. The cause of this phenomenon deserves inquiry.

Converted into a yellow oxide.

This experiment teaches us, that the molybdena had been changed by the action of sulphuric acid into a yellow oxide, containing more oxigen than the green and the blue, which passed to the state of green oxide in consequence of a disoxidation produced by the contact of metallic molybdena.

With nitric acid.

Exp. 30. In treating on the oxigenation of molybdena, I have already had occasion to say something of the action of nitric acid on this metal. The experiments I shall relate will serve as a continuation. Ten grains of powdered molybdena were put into two drachms of nitric acid diluted with an equal quantity of water. At the expiration of a quarter of an hour there was a slight evolution of nitrous gas, and a pale red solution was formed. To accelerate the action of the acid I employed a gentle heat, when the molybdena soon disappeared, and the liquor assumed a yellowish brown colour with a tinge of red. I added ten grains of molybdena two different times; and when I had added the last ten grains, the liquor, which had been clear, grew turbid, and became of a carnation red. This, added to a slight evolution of nitrous gas, led me to conclude, that the acid was completely saturated. After standing a little while, blue oxide was perceived to form at the bottom of the vessel, where a little molybdena still remained undissolved: a phenomenon similar to that observed in the solution by sulphuric acid. Twenty-four hours after, the matter that rendered the liquor turbid was separated, and it comported itself in all respects like molybdic acid.

A solu-

A solution of molybdena by nitric acid, made without heat, became perfectly clear in a few hours, and of a yellowish brown inclining to red. It had a slightly acid taste, that left behind it a bitterness with somewhat metalline. Part having evaporated by a gentle heat in a porcelain capsule, it left a pulverulent residuum of a dirty reddish yellow, which, being put into a small quantity of water and shaken, was entirely dissolved, except a small portion, that was molybdic acid. The solution was yellow inclining to red: and on being digested with metallic molybdena it became blue. Solution in nitric acid without heat.

Twenty grains of powdered molybdena having been put into a drachm of fuming nitric acid, an extremely vivid effervescence took place, attended with an extrication of red vapours, and the mixture became consolidated into a mass of a light brownish red colour. Another drachm of the same acid being poured on this, and moderately heated, white molybdic acid was very readily produced.

Exp. 31. The reddish solutions obtained in the preceding experiments being filtered, ammonia, cautiously added, produced a flocculent precipitate of a brownish red colour, which, having been washed and dried, yielded a powder of a lighter hue interspersed with white and shining crystalline particles. Some of this powder was put into a small quantity of water, at a moderate temperature, and shaken, in which it all dissolved except a few small white crystals. These crystals however were not molybdic acid, for they were much more soluble, and had a much stronger acerb taste. The solution of the brown powder was of a vinous yellow colour inclining to red. The water, with which this powder was washed after its precipitation, had a deeper colour, because the precipitate was more soluble after being wetted. On adding ammonia, or potash, to the solution, a brown red precipitate fell down slowly once more. This precipitate being treated with a solution of alkaline carbonate, it was not attacked, but the white crystals were dissolved with effervescence. Farther experiments are necessary, to determine the nature of the products formed in this process. I shall here confine myself to the remark, that the brown precipitate cannot be taken for the brown oxide The nitric solutions examined.

oxide obtained by the decomposition of molybdate of ammonia, for this oxide appears to be insoluble in water: and besides, the precipitate does not furnish blue oxide with molybdic acid, but only with molybdena in the metallic state, which indicates a higher degree of oxigenation than that of the blue oxide.

With muriatic acid.

Exp. 32. Ten grains of powdered molybdena were put into a drachm of muriatic acid of the specific gravity of 1·135, and left for twenty-four hours. The acid exerted no action on the metal, it remained in the same state: and even after it had been boiled to dryness, a second drachm of acid added, and this boiled on it a few minutes, no effect was produced.

This fact appearing to me inconsistent with the property I had observed in metallic molybdena of being converted into blue oxide after having been simply wetted, I tried muriatic acid diluted with water. The metal however was not attacked, whether I employed one, two, or three parts of water to one of acid, and digested the metal in it for a long time, or boiled it.

Wetted molybdena not oxided by the water, but by the air.

Thus it appears, that, when powdered molybdena is simply wetted, the oxidation is not produced by the water, but by the oxigen of the atmosphere; the water serving only to conduct the oxigen, and dissolve the oxide formed, so that the metal continually presents a fresh surface to the action of the air.

Oxigenized muriatic acid.

Exp. 33. Ten grains of metallic molybdena were put into three ounces of water saturated with vapour of oxigenized muriatic acid: the mixture was shaken a little, and a blue solution void of smell was produced. But the greater part of the metal was not dissolved: nor was it by the addition of six ounces of acid. The liquor when filtered was of a fine blue colour; but on adding liquid oxigenized muriatic acid, the solution became as clear as water; and when more molybdena in the metallic state was put into it, the blue colour reappeared.

Arsenic acid.

Exp. 34. Ten grains of molybdena were put into a drachm of liquid arsenic acid containing half its weight of dry acid, and left to stand twenty-four hours in a closely stopped bottle. At the expiration of this time a thin stratum

tum of the liquor, about half a line thick, was of a brown yellow colour. Having boiled the mixture and evaporated to dryness, diluted the residuum in half an ounce of water, and shaken it slightly, I had a fine blue solution, and but little of the metal appeared to be left unaltered. Thus the metal had here been oxided at the expense of the arsenic acid, and converted into blue oxide.

Phosphoric acid.

Exp. 35. Ten grains of molybdena, half a drachm of phosphoric acid, and a drachm of water, were put into a phial, which was stopped close, and left to stand twenty-four hours. No effect was produced, and the mixture was boiled to dryness. When the residuum was nearly dry, a vapour exhaled, which had a little of the smell of phosphorus, accompanied with something like that of an alkaline lixivium when boiling down. The flame of lighted paper held over it assumed a greenish yellow colour. The residuum was heated red hot, but no stronger smell was given out, that could lead me to suppose the phosphoric acid had acted on the molybdena; and in fact when the mass was cooled and diffused in half an ounce of water, the greater part of the metal remained at the bottom, without having undergone any alteration. The supernatant liquor was of a yellowish brown colour, had a strongly acid taste, and left a metallic taste on the palate. A similar quantity of water was evaporated from the metal several times, but I did not observe the least change, and no blue oxide was formed. A small quantity of this solution was evaporated to dryness, and a grayish blue matter remained, which, on dissolving it again, to my great surprise assumed a yellowish brown colour. Ammonia added to the solution gave it a dull colour, without producing any precipitate: it was not till after the expiration of four and twenty hours, that a few brown flocks separated.

Boracic acid.

Exp. 36. Having treated molybdena in the same manner with boracic acid, at the end of a few hours the liquor assumed a slight blueish tint, which did not increase afterward, even when evaporated and the residuum again dissolved. Thus it appears, that boracic acid has no action on molybdena, and was not the cause of the slight blue colour observed.

Succinic, tartarous, citric, and acetic acids.

I had similar results with the succinic, tartarous, and citric acids: only I observed, that, in treating molybdena with succinic acid, the liquor became green during evaporation. Acetic acid produced no effect on it cold; but when boiled, and the liquor reduced to about half, it assumed a brownish yellow colour. Ammonia scarcely rendered the solution turbid.

General conclusions on the action of acids.

From what has been said it appears:

1st. That, whenever molybdena is dissolved by acids, it becomes oxided at their expense, and consequently can be dissolved only by those acids, which, like the nitric, sulphuric, oxigenized muriatic, phosphoric, and arsenic, are susceptible of several degrees of oxidation, and capable of parting with oxigen, either at the common or a higher temperature.

2dly. That by the action of acids molybdena may be brought to the state of blue oxide, and sometimes of brown, the nature of which is yet to be examined. The phosphoric alone appears to produce a different state.

3dly. That these solu ions can scarcely be considered as salts, on account of the acid nature of the oxide of molybdena.

IX. *Action of potash on the native sulphuret of molybdena.*

Potash with the native sulphuret of molybdena.

Exp. 37. On fifty grains of pure sulphuret of molybdena I poured a lixivium containing two hundred grains of pure caustic alkali, evaporated to dryness, diluted the residuum in water, and evaporated again. After repeating this several times, I separated the undissolved part by filtration, washed, and dried it. The loss amounted to scarcely four grains, and what remained had the same appearance as before. On this I poured sulphuric acid diluted in water, but no sulphuretted hidrogen gas was evolved. The filtered liquor has a strong taste of sulphurous acid; diluted sulphuric acid expelled from this a large quantity of sulphuretted hidrogen gas; its colour, which was a pale brownish yellow, changed to brownish red; and at the end of a few minutes a fine brownish red precipitate was formed, which gradually changed brown, and thence to a yellowish brown; the liquor becoming a pale reddish brown. The

precipitate

precipitate when dried was of a chocolate colour, and weighed $3\frac{1}{2}$ grains. This appeared to be a simple hidrothianat [hidrosulphuret] of molybdena: for when heated with muriatic acid a small quantity of sulphuretted hidrogen gas is evolved, and when heated redhot in a crucible it does not give out the blue flame of sulphur, but simply a smell of sulphurous acid. On decomposing it with nitric acid, it immediately gave out sulphuric acid, which was rendered evident by barytes.

Exp. 38. Twenty-five grains of sulphuret of molybdena were put into a lixivium containing a hundred grains of caustic alkali, evaporated, and heated redhot for a quarter of an hour. As soon as the alkaline mass began to flow, the alkali acted so powerfully on the molybdena, that the whole of the metal seemed to be fused by it. The mass had assumed a cherry red, which soon passed to a deep crimson. The water in which this was diffused acquired a deep green colour, which it lost in a few hours by mere exposure to the air, and became of a blackish gray. The residuum, after being washed and dried, was of a light gray colour, and weighed twenty grains. Its nature will soon appear.

Heated redhot and fused.

Part dissolved.

Residuum.

Sulphuric acid and muriatic acid diluted with water, and added in excess to the solution that had passed the filter, extricated from it sulphuretted hidrogen gas, and occasioned a precipitate similar to that of the preceding experiment. A part of the molybdena formed with the free acid a blue solution above the precipitate. Nitric acid occasioned a similar precipitate: but the blue liquid, that contained it, became greenish, and afterward of a reddish yellow, in consequence of the progressive oxidation.

Solution examined.

The experiments related indicate, that the alkali (potash) exerts but little action on molybdena in the dry way, and still less in the wet. I thought, that, if the quantity of sulphur were increased, the action might be more considerable, and accordingly I made the following experiment.

Potash acts but little on it.

Exp. 39. I took ten grains of powdered molybdena, which I put into half an ounce of an alkaline lixivium holding in solution twenty grains of sulphur. This I boiled and evaporated almost to dryness twice. The matter, as in the 38th experiment, was of a cherry red round the edge.

Treated with potash and sulphur.

On diffusing it in water the solution was of a fine deep green. The molybdena however did not appear to be attacked in any sensible degree. Forty grains of sulphur were added, and the process as above was repeated three times. The molybdena was still found to be but little altered, and had lost only two grains. The solution being decomposed by sulphuric acid, it yielded only a grayish blue precipitate, the aspect of which was perfectly like what is called *lac sulphuris*, and contained a few flocks of a yellowish gray.

In the dry way.

Exp. 40. I then took two drachms of alkaline lixivium, thirty grains of sulphur, and ten grains of molybdena; put the whole into a Hessian crucible; evaporated to dryneſs; and left it in a red heat for a quarter of an hour. The mass being diffused in eight ounces of water, and filtered, the undissolved residuum weighed three grains. The solution was of a fine yellowish red, and sulphuric acid produced in it a blackish brown precipitate, which was in no respect altered by an excess of the same acid: the liquor gave no sign of a blue appearance: and the precipitate, after being separated, washed, and dried, was of a brownish black, and weighed forty-five grains.

Precipitate examined

This precipitate was not altered by boiling in sulphuric acid, and afterward with muriatic: but when nitric acid was added to the muriatic, and it was boiled again, it was decomposed and dissolved, with the exception of a little sulphur. A solution of barytes indicated the presence of sulphuric acid. Five grains of the precipitate, having been heated redhot in a small glass, gave out about two grains of sulphur. The residuum was speedily oxided by nitric acid, but still a little sulphuric acid was found in the solution, which proves, that the action of the fire had not separated all the sulphur. From what has been said it follows, that the precipitate was composed of molybdena in the metallic state, or approaching to it, of hidrothianat of sulphur, and of a slight excess of sulphur; while the precipitates in the 36th experiment were composed of oxide of molybdena combined solely with sulphuretted hidrogen, or at most with a little sulphur. This experiment having been repeated with four times the quantity of molybdena, and the

the roasting continued a quarter of an hour longer, gave the same results.

X. *Action of hidrothianates of alkaline sulphurets, and of pure hidrothian acid, on molybdic acid.*

Exp. 41. I dissolved molybdate of ammonia in twenty times its weight of water, and added sulphuric acid, till the precipitate formed was entirely redissolved. I then poured in hidrothianate of ammoniacal sulphuret, and a reddish brown precipitate was formed, which was more or less considerable, and the supernatant liquor was more or less blue, according as the quantity of sulphuric acid and of water employed to dissolve it was greater or less. I found too, that on adding a small quantity of hidrothianate of ammoniacal sulphuret to the solution of molybdate of ammonia, the sulphuric acid produced no precipitate, but merely rendered the solution blue; while a precipitate took place, if there were a larger quantity of hidrothianate of ammoniacal sulphuret: thus in one case all the hidrothianate of sulphur is employed in disoxiding the molybdic acid.

Solution of molybdena in sulphuric acid treated with hidroguretted sulphuret of ammonia.

Exp. 42. Five grains of sublimed molybdic acid dissolved in ten drops of concentrated sulphuric acid were put into five ounces of water. Hidrothianate of sulphuret of ammonia occasioned in this a chocolate coloured precipitate, which was almost black when dried. An excess of acid did not decompose it, or produce a blue colour: thus it was similar to the native sulphuret of molybdena.

Molybdic acid.

Exp. 43. Molybdate of ammonia was dissolved in twelve times its weight of water, sulphuric acid added in excess, and solution of sulphuret of potash poured in. This occasioned a light reddish brown precipitate, and the liquor became blue. Sulphuric acid being added to a solution of molybdate of ammonia merely to saturation, hidrothianate of sulphuret of potash occasioned a flesh coloured red precipitate inclining to a copper colour. In a solution to which no sulphuric acid had been added, no precipitate was occasioned by the hidrothianate, the liquid merely becoming a little milky, which might be expected from the property I have already observed the sulphuret of potash possesses of dissolving molybdena. The acid added afterward produced anew

Molybdate of ammonia with sulphuret and hidroguretted sulphuret of potash.

anew a precipitate of a reddish brown colour. All these precipitates were decomposed by an excess of acid, a blue solution was formed, and nothing remained at bottom but sulphur of a brownish gray colour containing a little molybdena.

Sulphuretted hidrogen gas passed into a solution of molybdic acid.

Exp. 44. Two phials were connected together as in Woulfe's apparatus. In one, which served as a receiver, there was a solution of one drachm of molybdic acid in eight ounces of water: in the other there was an ounce of sulphuret of lime with eight ounces of water, and sulphuretted hidrogen gas was evolved. As soon as the gas began to pass through the solution, this assumed a reddish brown colour, which became deeper and deeper, but still continued clear. I took a little, which smelled strongly of sulphuretted hidrogen, added to it some muriatic acid, and a blackish precipitate was formed. At the expiration of four and twenty hours the whole of the solution became a little turbid; and after exposure to the air for twelve hours in shallow vessels it was completely turbid, opake, and of the colour of mud. Heated afterward to ebullition, it resumed its clearness and colour, except that it was a little more inclining to yellow. The froth that formed during the boiling was of a fine reddish yellow, like tincture of saffron. While it was evaporating to dryness by a moderate heat, a smell of sulphuretted hidrogen continued to be given out, and toward the end a great deal of ammonia was evolved.

Brown residuum examined.

The residuum, weighing fifty-five grains, was of a light brown chocolate colour, and exhibited the following properties. 1. Ten grains being exposed to a moderate heat, a pretty large quantity of ammonia was evolved, accompanied with a smell of sulphuretted hidrogen. This smell alone was perceived when the heat was increased: at length sulphurous acid was given out, and the matter assumed a blueish black colour. It now weighed eight grains, was insoluble in water, and in the acid a little concentrated by a mean temperature. Thrown into a redhot crucible, it immediately became red, sulphurous acid vapours were expelled, and it melted. This was molybdic acid. 2. Ten grains of the residuum put into a drachm of muriatic acid, and heated to ebullition, gave out but little sulphuretted hidrogen; and formed

formed a brownish yellow solution, which on dilution with water assumed at first a blueish green colour, and afterward became completely green. A similar quantity having been previously shaken in water, and afterward put into muriatic acid, gave rise to a pretty considerable evolution of sulphuretted hidrogen, and produced a blue solution, which soon assumed a tinge of green, and let fall a blue precipitate insoluble in water. This I had an opportunity of observing in several experiments. Its external appearance greatly resembles that of the blue oxide of molybdena, from which it differs however, since it is not soluble like it in water. It requires farther examination therefore, to determine its nature. 3. Five grains of the dried residuum were put into half an ounce of cold water, and shaken; but no effect was produced. Being boiled for a quarter of an hour, part was dissolved, leaving two grains of a fine reddish yellow colour. The solution had the same colour as the preceding: it emitted a strong smell of sulphuretted hidrogen: the sulphuric acid increased this smell, and changed the solution at first blue, afterward green.

A compound of sulphuretted hidrogen, ammonia, and molybdena.

From all these circumstances it appears, that the residuum is a triple compound of hidrothian acid, ammonia, and molybdena. With respect to the acids it comports itself like the precipitates obtained in the experiments 38, 41, and 43. After being roasted to redness it approaches the native sulphuret of molybdena, from which however it appears to differ still by retaining a small portion of sulphuretted hidrogen. It is much more quickly converted into acid by the action of fire than the sulphuret of molybdena.

Sulphuretted hidrogen gas passed into water in which undissolved molybdic acid was diffused.

Exp. 45. Ten grains of very pure molybdic acid, first fused, then powdered, and afterward boiled in ten ounces of water, which dissolved but a very small part, were put into the same apparatus as that of the preceding experiment, and subjected to the same treatment. As soon as the sulphuretted hidrogen gas began to pass over, the liquid became brown: the colour grew deeper and deeper, and the greater part of the molybdic acid, which swam in the solution, was dissolved: nothing remained at the bottom but some brownish black flocks. At the conclusion the liquor assumed the same colour as in the preceding experiments, and

and had a strong smell and taste of sulphuretted hidrogen. At the end of four and twenty hours it became turbid, and deposited a pretty considerable quantity of a yellowish brown powder, which was separated and dried, when it became of a brownish black. The filtered liquor was of a yellowish brown: when made to boil, sulphuretted hidrogen gas was evolved; a larger quantity of powder was precipitated; and it retained but a slight smell of sulphuretted hidrogen, which the addition of a few drops of muriatic acid rendered stronger, at the same time producing a blue colour. The precipitated powder, put into muriatic acid and exposed to a moderate heat, comported itself like the residuum of the preceding experiment; but boiling it ultimately produced a solution of a brownish yellow colour. A little of this powder, being thrown into a redhot crucible, burned immediately with a sulphurous flame, which soon disappeared.

Pure molybdic acid will combine with sulphuretted hidrogen.

This experiment shows, that pure molybdic acid is likewise capable of combining with hidrothian acid; but this combination is not as constant as that of the preceding experiment, in which ammonia too is present. It proves the variations, that the less limited disoxigenizing action of the hidrothian acid must produce. Thus by dessication simply it passes to the same state, to which the compound of the preceding experiment is not to be brought but by a much stronger heat; and by the oxidation of a part of the hidrogen it forms a hidrothianate of sulphuret of molybdena, that gives out in roasting a vivid sulphurous flame, which the native sulphuret of molybdena does not, and is converted into molybdic acid.

It remains for me yet to examine the action of hidrothian acid on molybdena in the same respects as in the 41st experiment.

Molybdate of ammonia decomposed and redissolved by sulphuric acid, and treated with sulphuretted hidrogen.

Exp. 46. Sulphuretted hidrogen was passed in the manner already mentioned through a solution of a drachm of molybdate of ammonia in four ounces of water, which had been decomposed and redissolved by three drachms of rectified sulphuric acid. In two or three minutes the solution, which was before like water, assumed a blue colour. Five minutes after a light chocolate brown matter was deposited

on

on its surface, and on the wet sides of the vessel; but this disappeared after a few minutes. The fine blue colour of the solution changed to a black, and a precipitate of the same colour fell down. The liquor having been filtered, and set on the fire, became again of a fine blue by boiling. The water with which the precipitate was several times washed was also blue, but the colour had little intensity. The precipitate, having been dried, was of a blueish black, and exhibited the following results. 1. Boiled in moderately concentrated muriatic acid, it yielded a brownish yellow solution. 2. Thrown into a crucible at a dull red heat, it burned with a fine blue flame; which in a crucible at a bright red heat was quickly over, but there was a very considerable extrication of sulphurous acid after the flame had ceased. The residuum left after the combustion with flame was of a blackish brown, insoluble in water, and reducible to molybdic acid by increasing the heat. Put into water and shaken it gave a light blue tinge after some time. The residuum separated by the filter had lost its brown hue, and appeared almost entirely black. These experiments indicate, that the molybdic acid had been at first disoxigenized, and that afterward it entered into combination in the brownish black precipitate, which appeared to contain a little blue oxide; a circumstance that seems peculiar in this case, and merits investigation; but which in other respects comported itself as in experiment 45.

From all the experiments repeated under the 9th and 10th heads it follows: 1st. That potash exerts scarcely any action on sulphuret of molybdena in the humid way; that this action is more considerable in the dry way; and that in dissolving afterward in water a greater or less combination of sulphuretted hidrogen with sulphur takes place.

General remarks on the action of potash,

2dly. That the sulphuret of potash comports itself in the same manner. From compounds formed in the dry way acids precipitate a matter, which is a sulphuret of molybdena containing a small portion of sulphuretted hidrogen, and which comports itself with acids nearly as native sulphuret of molybdena.

sulphuret of potash,

3dly. The hidroguretted alkaline sulphurets precipitate from the solution of molybdic acid a matter of a colour similar

hidroguretted alkaline sulphurets,

milar to that of chocolate, which imparts a blue colour to the acids in which it is dissolved, and appears to differ from the preceding in the oxidation of the molybdena, and in containing more sulphuretted hidrogen and less sulphur. Thus we have two compounds of this kind; and the latter appears capable, under certain circumstances, of being converted into the former.

and sulphuretted hidrogen.

4thly. Pure sulphuretted hidrogen gas equally combines with molybdena, exhibiting phenomena that indicate a disoxigenation, and forms products similar to those resulting from their combinations. The passage of this gas through a solution of molybdate of ammonia gives rise to a triple compound, which is soluble in water, decompofable by heat, and rendered by it similar to the native sulphuret of molybdena.

Conclusion.

I here conclude the publication of my experiments on molybdena. I am free to confess, that they do not exhibit a complete work; but I flatter myfelf, that some conclusions may be drawn from my labours not altogether unimportant to the science of chemistry. Besides it was necessary, that such experiments should be some time made; and I can aver, that I employed all the care and attention possible, so that complete reliance may be placed on their accuracy. Farther experiments will complete what I have begun.

Farther experiments promised.

These I shall undertake, as soon as I have procured a sufficient quantity of molybdena, and my occupations will afford me leisure.

III.

On the native Gold Duft found in the Hills in the Environs of the Commune of St. George's, in the Department of the Doire: by Mr. GIULIO, *Prefect of the Department of the Sesia*.*

Native gold found in the sands of rivers.

IT has long been known, that a great number of rivers and brooks carry with them particles of native gold, of larger

* Journal des Mines, No. 116, p. 145.

larger or smaller size: that independently of the places where this metal is found in its native situation, it is disseminated in grains in their sands, as those of the Rhone, the Arriège, and the Cèze in France, and with us in those of the rivers Doire, Balthée, Cervo, Elbo, Mallon, and Orba, and of the rivulets Oropa, Orémo, Evançon, Vison, &c. It is equally known, that there are persons, who make it their whole business, to search for this gold.

Mineralogists are not perfectly agreed respecting the origin of this gold dust; for the oldest, and among the moderns Brochant, suppose this gold was originally brought from mines, commonly situate in primitive mountains, from which it has been washed down by the water of the rivers. "Native gold," says Brochant*, "is found chiefly in primitive mountains, where it is met with in veins, and sometimes disseminated in the rock····it occurs also in alluvial strata, where it is frequently wrought with advantage. The sand of several rivers is mixed with grains of gold, which are separated from it by washing. It is unquestionably evident, that the gold here is met with accidentally; and that it is deposited by the water, that has washed it away from its original situation, which was probably the same as is indicated above." **Its origin disputed.** **Brochant.**

Others on the contrary think, that these metallic particles were originally disseminated in auriferous strata, in the very places where they are exposed to view by great floods, or overflowings of the rivers, or that they have been washed into the latter by torrents in storms or heavy rains. **Supposed to be in their original situation.**

I do not intend to enter into the question generally, or at large. This I leave to the learned, whose chief study is the improvement of the science of mineralogy. My inductions go no farther than the small number of researches I have made: yet I think I may venture to say, from the observations I am about to present the reader respecting the locality and situation of the native gold dust in the commune of St. George's, that such dust is not always washed down from mines in the mountains by rivers. And if such **At least not always washed down from mines.**

* Elementary Treatise on Mineralogy, according to the Principles of Prof. Werner. Vol. II.

were

were the primitive origin of their dissemination amid the strata, it certainly could have happened only at some very remote period of the grand disruptions, that have taken place on the surface and exterior strata of our globe. But these revolutions, of which we have no records, are buried in the night of time. For we shall see, that strata, which furnish gold dust, are found at a considerable depth in some hills, equally remote from mountains capable of furnishing it, and from rivers that could force it from its native situation. Consequently it could have mingled in them only at a very distant period, when the strata of the hills assumed the arrangement they have at present, namely, at the time of their formation.

This the opinion of many.

Such too has been the opinion of several naturalists of our country, and I should be guilty of injustice to them, if, in collecting fresh proofs tending to support their hypothesis, I omitted the mention of their valuable works. Accordingly I shall quote Mr. de Robillant, who, speaking of the gold dust found in the sands of the Orco, says very positively: "this river carries along gold, which the people of the country observe only below the bridge down to the Po; which confirms the opinion held by the people best acquainted with the natural history of the country, that it is from the gullies and hills that this gold dust is washed down into the river by the rapidity of the water during storms---*. This valuable metal does not come from the high mountains, since none is found above the bridge, but it originates from the washing of the red earth, of which most of these hills and plains are composed, and which in stormy weather is carried down into the principal river†.

De Robillant.

Balbo.

Mr. Balbo generally adopts the explanation of Mr. de Robillant respecting this species of native gold, in his learned Memoir on the auriferous sand of the Orco. "Every one," says he, "knows, that gold dust is collected in the Orco······But I do not believe it is equally known, that gold is found, not in the bed of the river alone, but to the

* See a geographical Essay on the continental Territories of the King of Sardinia, by de Robillant, in the Memoirs of the Royal Academy of Sciences of Turin for the years 1784,5, Part II, p. 234.

† Ib. p. 268.

distance

distance of several miles, every where mingled more or less with the sand......It is very positively asserted, that it occurs in all the little rivulets between Valperga and Rivara ......I endeavoured to discover, whether all the waters rise sufficiently near to each other, to lead us to suppose, that they equally derive their gold from the same mine: as it is in this way that the vulgar, and even most of the learned, generally account for the gold found in rivers. But I was completely convinced, that the waters of which I speak arise from different heights at some distance from one another, so that, as we cannot suppose all these places to contain mines, from which the gold may be derived, we must necessarily admit, that the particles of gold are not separated daily by the action of the water, and carried along by its streams, but that the water finds them in the soil itself over which it flows......And it is farther confirmed by the observation, that the auriferous strata disappear as we proceed up the Orco; that we find them at farthest only as high as the bridge; that above this all traces of them are lost, though this is very far from the springs, while as we descend into the plain these strata are every day exposed by the action of the water, and particularly in floods *."

Found in the earth several miles round a river.

In a second part I shall speak of the theory proposed by Mr. Napion, in his Memoir on the Mountains of Canavais †, who, having observed that all the pyrites of those mountains are auriferous, attributes the particles of gold to their decomposition or attrition. This is the opinion of our worthy colleague, Dr. Bonvoisin.

Supposed to come from pyrites.

The observations I am now about to communicate appear to me still more decisive, than the proofs alleged by these authors; and if the earths of which I shall speak do not furnish so large a quantity of gold dust, they afford indisputable arguments, to convince us, that the gold certainly does not proceed from any mine traversed by water, at least in the present day.

Further proof of its origin to be adduced.

* Mem. of the Roy. Ac. of Turin for 1784,5, on the auriferous Sand of Orco, Part II, p. 404, 407.

† Ib. for 1785,6, p. 345,6.

On

Hills in the department of the Doire.

On the north of the commune of St. George's, in the circle of Chivas, in the department of the Doire, we find fertile rising grounds, and hills almost wholly covered with vineyards, which continue till we come to the highest of them, the hill of Macugnano, part of which is cultivated, part covered with wild chestnut trees; a distance of about three miles.

Three distinct strata.

In proceeding from the outer and upper surface of these hills to the bottom of the valleys, which intersect them in different directions, we find in general three very distinct strata.

The upper.

The upper stratum is for the most part argillaceous, as it furnishes an excellent earth for making bricks and tiles. The thickness of this stratum varies in different places from three or four feet to twenty-five or thirty. The second stratum, which stretches likewise horizontally beneath the stratum of clay, is a few feet thick. It is composed of a considerable portion of sand, of gravel, and of pebbles of different natures, argillaceous, calcareous, and quartzose. Of these I shall speak more particularly in the second part, as well as of the fragments produced by their being broken or decomposed. The third or lower stratum, which forms the bed of the valleys, and of the rivulets that run through them in rainy weather, is composed in great measure of the fragments of the argillaceous and calcareous stones of the second stratum.

The middle.

The lower.

Valleys produced,

The rains gradually produced little gullies in different directions; which by the falling of fresh rain, and the quantity and rapidity of the water, have in the course of time been extended and converted into valleys, more or less broad and deep, in different places. Part of the water of several gullies accumulates particularly in one valley, where during storms and long rains it forms a torrent, called in the country the Merdanzone. Now the gold dust is found chiefly among the sands of this torrent, and of the small lateral rivulets, that flow into the Merdanzone or other similar valleys.

and gold found in them.

But does this gold proceed equally from the different strata I have mentioned above, or from one of them only? I first examined the brick earth, that of the upper stratum, in

None in the

in different places and at various depths: I also examined considerable depositions of this earth accumulated in the shallow valleys: but I never discovered the smallest particle of gold in it. The searchers for gold knows this so well by long experience and a great number of fruitless trials, that they never pay any regard to this stratum. It is the stratum beneath this, that composed of gravel, sand, and broken stones, in which the particles of gold are found.

upper stratum

but in the middle one.

Of this I have convinced myself by repeated trials: and though in general, if equal quantities of earth be taken from this stratum, and from the bottom of the torrent or rivulets flowing into it, the latter will yield most gold, it scarcely ever happens, that no gold is found in the latter upon trial. The particles of gold obtained from the auriferous stratum itself, which have not yet been rolled along with the sand by the rains, have a duller and deeper yellow colour than those collected in the bed of the torrent, or of the rivulets, which are of a more shining yellow, no doubt in consequence of the attrition. They are generally found amid a sand, that is more or less fine and blackish, and apparently of a siliceous and ferruginous nature. The earth of the same nature, which reaches to some distance, equally contains gold. Thus a brook that runs on the east of the commune of Aglie, between the mansion and the park, and receives the rain water that washes down an earth composed of different strata of the same nature as those of the auriferous hills of St. George's, equally rolls along particles of gold disseminated beneath the argillaceous stratum, which in certain places is of very considerable thickness.

Most in the beds of rivulets.

This distinguishable from the other.

Fifteen or twenty years ago several persons in the commune of St. George's made it their principal employment, to search for gold in the sand of the torrents and rivulets that I have mentioned. This they did particularly after or during heavy rains, and after storms.

Formerly collected

The quantity of gold they collected in a day was very variable. Sometimes each of them would gain eight or ten shillings a day, at other times scarce a fourth or fifth of this sum. The size of the particles too varied much, as from an almost invisible atom to the weight of nine or ten grains or more

with some profit.

more. They were afterward sold to merchants, who sent them to the mint.

Gold found in other situations.

I am not speaking here, as is obvious, of gold dust disseminated in arable land. Earth of this kind in the territory of Salussole, as I am informed by my colleague Giobert, contains particles of gold. The earth of gardens is known to contain them. It has been proved in our days by the experiments of Sage, Berthollet, Rouelle, Darcet, and Deyeux, that there are particles of gold in vegetables. Berthollet has extracted about 2·14 gram. [33 grs.] from 48900 gram. or a hundred weight of ashes.

Here not on the surface, but under it, sometimes to a considerable depth.

Hitherto gold has not been found in the arable land in the environs of St. George's, but only in the stratum beneath the clay, the surface of which is cultivated. The auriferous stratum, as I have observed, is more than thirty feet deep below the argillaceous stratum in some places.

Every where it has a common origin.

We have nothing to do here with particles of gold mixed with the surface mould by the decomposition of plants, or which plants have derived from the earth. I have no doubt, that the particles of gold found in the environs of St. George's have the same origin as those met with from Pont to the entrance of the Orco and of the Mallon into the Po, from Valperga and Rivara to Aglie and St. George's; as well as of those, which Dr. Bonvoisin observed in the environs of Challant in the valley of Aoste. The famous piece of native gold preserved in the arsenal was found there. In that space pieces of gold of the weight of a louis have sometimes been found; and other pieces are mentioned of the value of more than 100l. [£4 3*s.* 4*d.*]. It is probable, that the gold found in the earth in the valley of Brozzo, and in other places, has the same origin. I shall propose my conjectures on this subject in the second part of this memoir, where I shall enter more at large into the nature of the earths and stones of the auriferous strata, as well as the nature of the land in which they are contained.

IV.

IV.

Calculution of the direct Attraction of a Spheroid, and Demonstration of CLAIRAUT'S *Theorem. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

THE same mode of calculation, by which the figure of a gravitating body, differing but little from a sphere, has been determined (p. 208 of this volume), is also applicable to the magnitude of its immediate attraction, or the comparative length of a pendulum in different latitudes. Extension of former researches.

Suppose a sphere to be inscribed in the spheroid, and another to be circumscribed about it; I shall first show, that the attraction at the pole is equal to that of the smaller sphere increased by $\frac{4}{15}$ of that of the shell, and at the equator equal to that of the larger diminished by $\frac{2}{15}$. If we call the attraction of this shell 2, its surface being equal to the curved surface of a circumscribing cylinder, the attraction of a narrow ring of this cylinder, or of the elevated portion of the spheroid at the equator, supposed to act at the distance of the radius, or unity, may be expressed by its breadth: but in its actual situation its attraction in the direction of the axis is reduced in the ratio of the cube of the chord of half a right angle to the cube of the radius; and the attraction of any other ring will be to this in the ratio of the quantity of matter, or the cube of the sine of the distance from the pole, and of the versed sine directly, and in the ratio of the cube of the chord inversely; that is in the joint ratio of the cube of the cosine of half the angle and the versed sine: thus, if we call the cosine of half the angle x, the versed sine being $2-2x^2$, and the fluxion of the arc $\frac{2\dot{x}}{\sqrt{(1-xx)}}$, the fluxion of the force at the equator will be $\frac{1}{2\sqrt{2}} \cdot \frac{2\dot{x}}{\sqrt{(1-xx)}}$, and elsewhere as much less as Attraction of the prominent parts.

$x^3(2-2x^2)$ is less than $\frac{1}{2\sqrt{2}}$, that is, $\frac{4x^3\dot{x}}{\sqrt{(1-xx)}}-\frac{4x^5\dot{x}}{\sqrt{(1-xx)}}$, of which the fluent is found as before $(\frac{4}{5}x^4-\frac{4}{15}x^2-\frac{8}{15})\sqrt{(1-xx)}$; and this becomes $\frac{8}{15}$ while x increases from 0 to 1, being to 2, the attraction of the whole shell, as $\frac{4}{15}$ to 1; but if the radius of the sphere be 1, and the ellipticity e, the attraction of the shell will be to that of the sphere as $\frac{3e}{n}$ to 1, n being the mean density of the sphere, compared with that of the superficial parts, and the attraction of the spheroidal prominence will be expressed by $\frac{4e}{5n}$, that of the sphere being unity.

Polar & equatorial attraction.

The depression below the circumscribed sphere is equal, on the meridian, to the elevation above the inscribed sphere: but vanishes at the equator, being every where proportional to the square of the sine of the latitude; so that the mean depression of each of an infinite number of rings, of which any point of the equator is the pole, must be half as great as the elevation of the corresponding rings parallel to the equator; and the whole deficiency is equal to half of the whole excess, that is, to $\frac{2e}{5n}$: consequently, the remaining attraction of the shell is $\frac{13e}{5n}$, from which we must deduct the diminution of the attraction of the inscribed sphere $2e$, and the whole will become $1+\frac{13e}{5n}-2e$, which subtracted from $1+\frac{4e}{5n}$ leaves $2e-\frac{9e}{5n}$ for the excess of the immediate attraction at the pole above the equatorial attraction; to which if we add the centrifugal force f, the whole diminution of gravity g will be $2e-\frac{9e}{5n}+f$; but since e was before found to f as 1 to $2-\frac{6}{5n}$, or $=\frac{5n}{10n-6}.f$, we have $\frac{10n-9}{5n}.e=\frac{10n-9}{10n-6}.f$, and

and $g = \frac{20n - 15}{10n - 6} \cdot f$, to which if we add e, we find $e + g = \frac{25n - 15}{10n - 6} \cdot f = \frac{5}{2} f$; and this is the celebrated theorem of Clairaut.

It remains to be shown, that the diminution of the attractive force at different parts of the spheroid varies as the square of the cosine of the latitude. The elevation, being every where proportional to the square of the distance from the axis, may be divided into two parts; one proportional to the square of the sine of the distance from the meridian of the place, and the other to the distance from the plane of another meridian perpendicular to it: but the first of these being constant, whatever may be the position of the place to be considered, the second only produces the variation. Now if we take in the second portion the mean of the elevations at any two points of a less circle equidistant from the meridian, it will be proportional to the sum of the squares of the distance of the centre of the circle from the axis, and of the cosine of the distance from the meridian in the same circle, reduced to a similar direction, that is, diminished in the ratio of the radius to the sine of the latitude, since twice the sum of the squares of any two quantities is equal to the sum of the squares of their sum and their difference. We have therefore two quantities, varying as the square of the cosine, and as the square of the sine of the latitude respectively: but the square of the sine may be represented by a constant quantity diminished by the square of the cosine: and the decrease of the attraction of the inscribed sphere is as the elevation, which is as the square of the cosine; the centrifugal force reduced to a vertical direction is also as the square of the cosine. We have therefore, beside two constant quantities, two negative forces and a positive one, all varying as the squares of the cosine of the latitude; and it is obvious, that the joint result of the whole, or the upper real diminution of gravity, must also vary in the same proportion.

Variation of gravity.

A. B. C. D.

29 *June*, 1808.

V.

Reply to Professor VINCE'S *Ultimatum. By a Correspondent.*

To. Mr. NICHOLSON.

SIR,

IT is no unusual expedient with an expert disputant, to affect a contempt for his antagonist, which he does not feel; and to decline a contest, to which he is unequal, on the pretence, that it is superfluous to engage in it. I am far from wishing, to protract a controversy with Professor Vince; but I protest against his right to excuse himself from the necessity of replying to any future observations of mine on the ground of his engagement not to trouble you further on the subject. If however the Professor thinks my remarks undeserving of any additional notice from himself, it is to be presumed, that some person will be found among the numerous disciples of that illustrious school, in which he holds so distinguished a situation, who will undertake the easy task of confuting me, and vindicating the honour of the university from the slightest shade, that the publicity of such a mistake as I have imputed to Professor Vince could possibly cast on it.

Silence not to be allowed as an argument.

I grant, that *three quantities* are "put down" in the essay, which the Professor *now calls* the first terms of three series: but I still shall deny, that these series are to be found, or are any where mentioned, in that work. It is not a little remarkable, that a man, whose life is devoted to the science of reasoning with accuracy, should adduce so weak a proof of my being guilty of misrepresentation.

Series not mentioned.

With respect to the sufficiency of each of these quantities for determining its share of the force, Professor Vince's words are, p. 19: "Now the terms omitted in the series are comparatively so extremely small, that if they were not considered, they could make no sensible alteration in the result." And now he accuses me of a second "unaccountable misrepresentation," for saying, that he has mentioned

Quantities employed said to be sufficient.

tioned the terms actually employed "as sufficient" for his purpose.

In the third place, he does not appear to be aware of the distinction between physical and mathematical accuracy. Physically speaking, the series "*may certainly*" vary as $\frac{1}{a^2}$, with as little sensible errour as the law of gravitation: mathematically speaking, we have not the slightest evidence, that the law of gravitation "*varies accurately*" as $\frac{1}{a^2}$, and in this sense the Professor's assertion is totally void of foundation.

Law of gravitation not a mathematical truth.

The change of the law of density of the medium at the surface of a planet, instead of being "inconsistent with Newton's hypothesis," is the simple and unavoidable consequence of it. Each particle of matter being supposed to induce a certain state of the medium around it independently of all others, so that the attraction may be produced alike in all circumstances, the state of the medium within the planet must necessarily be such, as to produce the joint effect of all the attractions; that is, the force must vary as $x\dot{x}$, and the density as xx or aa; the square of the distance from the centre; and this must be the immediate consequence of the same cause, that produces the usual variation of density with respect to a single particle. It may be said, that the operation of this cause is equally obscure with the ultimate effect of gravitation considered as independent of it; and I am perfectly ready to admit the objection. I am not defending the Newtonian hypothesis; I am only endeavouring to show, that Professor Vince has attacked it unsuccessfully, and has heaped errour upon errour in attempting to support his arguments.

Law of density necessarily changed within a planet.

I am, Sir,

Your very obedient servant,

8th *July*, 1808. DYTISCUS.

VI.

Question respecting the Ignition of Tinder by compressed Air. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Combustible substances ignited by condensed air,

AMONG the number of philosophical apparatus of modern invention, there are perhaps few which involve more interesting matter of inquiry, than an instrument lately contrived for setting fire to combustible substances by the agency of compressed air.

in a syringe of small size.

The little apparatus, which I have seen for this purpose, was in the possession of Mr. Accum, who showed me its surprising effects in igniting common tinder, and different species of fungi. This singular mode of producing fire is accomplished by the quick compression of the small quantity of air contained in a condensing syringe of small size.

What is the cause?

It might perhaps be matter of considerable importance, in a philosophical point of view, to ascertain what change the air undergoes during this operation; whether the effect produced is to be ascribed to the mechanical action of the air, or to a change of capacity induced by the rapid condensation?

Your remarks may tend to the elucidation of this very curious fact.

I am, Sir,

Yours respectfully,

Lincoln's Inn, July 15th, 1808. T. CLIFTON.

REPLY.

Apparently diminution of capacity.

This experiment, which is now of some standing, seems to depend on the diminution of capacity, produced by the sudden

Mr. Wright's Artificial Horizon

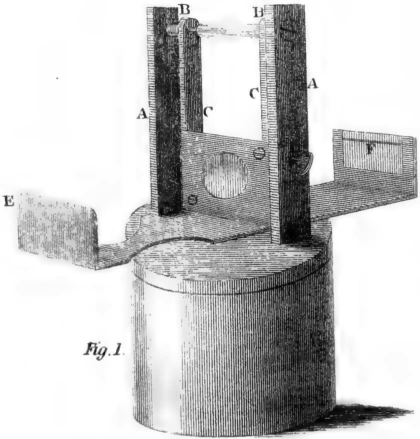


Fig. 1.

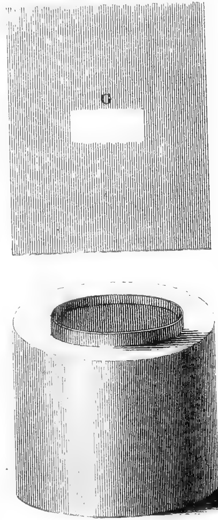


Fig. 2.

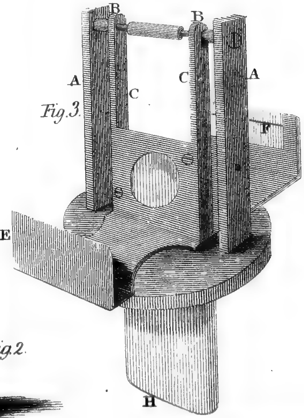


Fig. 3.

R. Roffe sc. 52 Queen Ann St. East.

sudden condensation. From Mr. Dalton's experiments, (see our Journal, vol. III, p. 160) it appears, that the condensation answering to the pressure of our atmosphere affords an increase of temperature upwards of 50 degrees; and, if we suppose this augmentation to be in the simple ratio of the compression, though it is probably higher, a compression of 18 atmospheres would give the temperature of ignition.

W. N.

VII.

Description of a portable artificial Horizon for taking Altitudes at Sea or Land, by Mr. WRIGHT. *In a Letter from the Inventor.*

SIR,

I Beg leave to transmit you a description and delineation of an artificial horizon, which I conceive will be found preferable to any other in use. Artificial horizon described.

Plate VII, fig. 1, represents the horizon with all its parts complete: fig. 2, a cylindric vessel of brass, to be filled with water when in use: fig. 3, the upper part of the horizon taken from the vessel, to show its internal parts. AA are two uprights of brass and a horizontal axis on the top, with two fine edges at BB, on which the brass frame CC is suspended. At the bottom of the frame are an index and two sights; the nearest sight E having a fine horizontal edge on its top, and on the farthest sight F is a fine black line in a piece of transparent ivory. The index is adjusted to a horizontal position by two screws, which fasten it also to the frame when adjusted; on the index is a convex glass to magnify the line on the ivory sight F, and throw its image on the edge of the sight E; and under the index, in its centre, is screwed a thin brass blade H, to be immersed in the water in the vessel, fig. 2, for the purpose of preventing the horizon from getting any vibrating or pendulous motion

to

to disturb its gravity, or divert the sights from their horizontal direction, when in use.

If the ship have a considerable motion, it is advisable, to suspend the box by a small brass gimmal at the top to a portable stand; and to prevent the wind's affecting it, a glass slides into each end of the box, through which the observation is taken; their surfaces being parallel, you adjust it by the sea horizon, or by meridian altitudes of the sun, the latitude being known, or by any other method observers make use of, or allow the index errour, as is done with the octants and sextants.

D is a small brass pin, to prevent the index from getting any motion in carriage; and is to be taken out, when the horizon is in use.

In observing the moon, planets, or stars, by night, a small lanthorn with a lamp is necessary, to be placed behind the box, so that the light may fall on the ivory, to show the line distinctly; and to prevent its spreading too much when you are observing the stars, the glass is to be taken out, and the brass with a small square hole, G, slid into its place.

Method of taking an altitude with it.

To take an altitude with the octant and artificial horizon, bring the eye as near to the horizon in the box as the frame of the octant will admit of, and in a horizontal line look at the fine edge of the sight E, which by the least motion of the head you may bring into contact with the line on the ivory sight F, and move forward the index on the frame of the octant with your hand, to bring the object you are observing to a coincidence with the ivory line also.

For altitudes of the sun or moon, and for all terrestrial objects, an octant of the usual construction will answer every purpose; but for observing the stars, one with a larger horizon glass, and its silvered surface also larger, with a different sight vane, would be preferable, and prevent your missing or mistaking a star when near to others, or its getting out of the glass in bringing into contact with the horizon. With such a quadrant and this horizon, the meridian altitudes of all bright stars, as they come to the meridian, may be taken, by which means the latitude might be frequently found by observations at night, and with as much ease as by the sun at noon-day; also, the altitudes of the moon

moon and stars, to correct the lunar problem for the longitude, will be more correctly and easily taken with it.

I am, Sir,

Your very obedient humble servant,

J. WRIGHT.

VIII.

*Description of an Apparatus to secure Persons from sinking in Water, or to act as a Life-preserver when shipwrecked, with instances of its Utility: by Mr. F. C. DANIEL, of Wapping**.

SIR,

I HAVE taken the liberty of sending one of my life preservers, and am proud to say, they have realized the name; and I shall feel myself obliged if you will cause it to be brought before the Society for their approbation. I beg to say, Sir, though I have given it publicity, it has never been before any committee. Life-preserver

I have enclosed a copy of a letter, which I received from the only surviving officer of the Alert private ship of war, and, independent of that document, I have had information from respectable authority, that the machines have saved several lives. has already saved several lives.

It is not, Sir, a pecuniary reward I look for, although I have sunk near £1500 in the undertaking; yet, I must confess, to have the sanction of the Society of Arts would be highly flattering, and the world from that moment must be convinced of their utility.

I have the honour to be, Sir,

Your obedient servant,

F. C. DANIEL.

* Abridged from Transactions of the Society of Arts for 1807. The gold medal was voted to Mr. Daniel for this invention.

Copy

Copy of a Letter from Mr. GEORGE WILLERS, *late Surgeon of the Alert, private Ship of War, lost off the Western Islands.*

SIR,

Privateer wrecked near the Western Islands.

I AM happy in having it in my power to say, I owe my life to your invaluable invention, the life preserver; and the circumstances relative thereto are as follow:—I shipped as surgeon on board the Alert, private ship of war, mounting 18 guns, and 98 men, commanded by James Desormeaux, esq., belonging to Messrs. Wright and Birch, Walbrook. We sailed from Falmouth, April 1805, and, after cruising five months, on the 22d of September, we unfortunately struck on a rock among the Western Islands, and the ship went to pieces in five minutes; at that time we had eighty-four men on board: I witnessed the loss of every officer, six in number, and sixty-four foremast men; thirteen of the crew were saved, by clinging to pieces of the wreck, spars, &c. which drifted from the wreck; and I have the happiness to say, by possessing one of your life preservers (though I cannot swim,) I was supported for some time, the sea running mountains high, but providentially a large Portuguese boat put off to my assistance, being then near a mile from the shore; and I was thus saved, by the hand of Providence and your invention, from a watery grave.

The surgeon saved by a life-preserver.

I beg, Sir, you will permit me to acknowledge how much I feel myself obliged to you; and you are at full liberty to make this case known for the benefit of mankind.

I am, Sir,

Your most obedient servant,

G. H. WILLERS.

Copy of a Letter from JOHN DICKENSON, *Esq. of the City of Norwich, to Mr.* DANIEL.

SIR,

I INTENDED myself the pleasure of calling on you, and acquainting you personally of a singular incident, when the

the excellence of your machine, or life preserver, was most conspicuously manifested.

A pleasure boat overset.

I went from the city of Norwich, in a pleasure-boat that I keep for the amusement of sailing, in company with a gentleman and two ladies. As our return to Norwich in the evening was indispensible, and the direction of the wind favouring us both ways, a few hours would effect it, the distance being only thirty miles: accordingly we set sail about four o'clock, it being moon-light during the night; and fortunately procured, in case of accident (the wind blowing hard at south-east) one of your life preservers, through the interest of a friend, of a captain, who had purchased one at Newcastle. The precaution proved, in a short time after sailing, to have been a fortunate one indeed. On tacking to enter Norwich river, at the extremity of a broad water, two miles over, known by the name of Braydon, a sudden gust overset the boat, precipitating myself, companion, and two ladies, into as agitated a water as I have ever seen at sea, (except in hard blowing weather). You may judge my situation at such a juncture. Your machine was jokingly filled as we came along, to which I ascribe (though very unexpected by us) our preservation. The gentleman, whose name is Goring, was inexpert at swimming, and with difficulty kept himself up, till I reached him; and then directing him to lay hold of the collar of my coat, over which the machine was fixed, I proceeded towards the ladies, whose clothes kept them buoyant, but in a state of fainting when I reached them: then taking one of the ladies under each arm, with Mr. Goring hanging from the collar of the coat, the violence of the wind drifted us on shore upon Burgh Marshes, where the boat had already been thrown, with what belonged to her. We got the assistance of some countrymen directly, (after taking refreshment at a marsh farmer's house, where we procured some dry clothing for the ladies, who were now pretty well recovered,) and by their endeavours put the boat in sailing trim, and prosecuted our voyage to Norwich, which we effected by eleven o'clock that night.

Two gentlemen and two ladies buoyed up by one life-preserver.

From this singular escape, on my return from Birmingham, I shall be induced to inspect your warehouse, and procure

procure the various prices of your invention, anxious to recommend it in even sailing excursions, in which its utility has been so evidently demonstrated, and its use ascertained.

You are at liberty, Sir, to make whatever use you please of this account, and I beg to subscribe myself,

Sir,

Your most obedient humble servant,

JOHN DICKENSON.

Swan with Two Necks, Lad Lane,
Jan. 30, 1807.

Reference to the figure of Mr. Daniel's Machine, called a Life Preserver when Shipwrecked, Pl. VIII, Fig. 1.

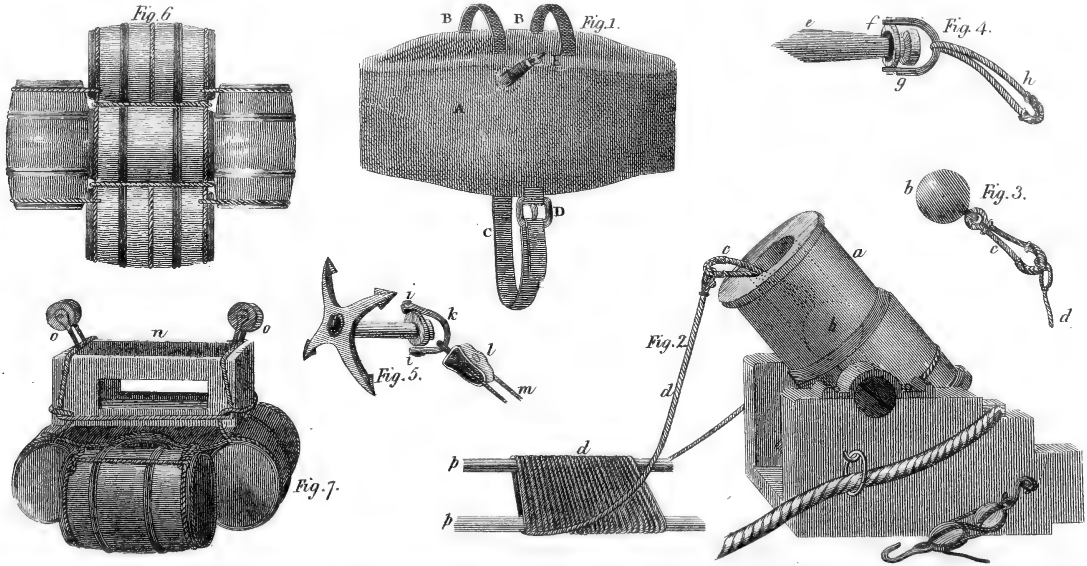
The machine described.

A, represents the body of the machine, which is double throughout, made of pliable water-proof leather, large enough to admit its encircling the body of the wearer, whose head is to pass betwixt the two fixed straps, BB, which rest upon the shoulders; the arms of the wearer pass through the spaces on the outside of the straps; one on each side, admitting the machine under them to encircle the body like a large hollow belt; the strap, C, on the lower part of the machine, is attached to the back of it, and by passing betwixt the thighs of the wearer, and buckling at D, holds the machine sufficiently firm to the body, without too much pressure under the arms. The machine, being thus fixed, is inflated with air by the wearer blowing in from his lungs, through the cock E, a sufficient quantity of air to fill the machine, which air is retained by turning the stop-cock. The machine, when filled with air, will displace a sufficient quantity of water, to prevent four persons from sinking.

Method of making it.

Mr. Daniel recommends his life preservers to be prepared as follows: viz. To select sound German horse-hides, and to cut a piece six feet long, and two feet six inches wide, free from blemish or shell; it is first to be curried, and then rendered water-proof by Mollerstein's patent varnish, of Os- born

Mr. Daniel's Life-preserver from Shipwreck.



Lieut. Bell's Method of saving Persons from stranded Ships. J. Roffe sc.

born street, Whitechapel, which preserves the leather more supple, and admits it to be easier inflated than any other water-proof leather.

The leather is to be nailed on a board, and the varnish applied upon it; it is then to be passed into an oven several times, the varnish being each time repeated, till the leather is completely covered; it is then cut in the form of a jacket, as above described, and neatly and firmly stitched; the seams and stitches are afterwards to be perfectly secured by the following black elastic varnish.

Take of gum asphaltum, two pounds; amber, half a pound; gum benzoin, six ounces; linseed-oil, two pounds; oil of turpentine, eight pounds; and lamp-black, half a pound; unite them together in an earthen vessel with a gentle heat. The machine, when properly made according to the drawing and description, resembles a broad belt, or circular girdle, composed of two folds of pliable leather attached together, and perfectly impervious to water. Varnish for the seams.

IX.

Account of Experiments made by Lieut. JOHN BELL, *of the Royal Artillery, to ascertain the Practicability of throwing a Line to the Shore from a Vessel Stranded*.*

THE several trials made before a Committee of the Society at Woolwich, on the 29th of August 1791, of throwing a line on shore on this principle, were as follow. Experiments.

From

* Trans. of the Society of Arts for 1807, p. 136. A publicity having been recently given to some experiments off the eastern coasts of this island, for preserving lives in cases of shipwreck, by means of a rope attached to a shell thrown from a mortar; the Society deemed it incumbent on them to remind the public, that, so far back as the year 1792, a bounty of fifty guineas was given to Mr. John Bell, then sergeant, afterwards lieutenant of the Royal Regiment of Artillery, for his invention of throwing a rope on shore, by means of a shell from a mortar, on board the vessel in distress; the particulars of which were published in the tenth volume of the Society's Transactions, page 204; but a descriptive engraving

A ball carrying a line thrown 400 yards from a boat.

From a boat moored about 250 yards from shore, the shell was thrown 150 yards on shore, with the rope attached to it; the shell was of cast iron, filled with lead, it weighed 75 lbs., its diameter 8 inches; the rope in the trial was a deepsea-line, of which 160 yards weighed 18 lbs; the angle of the mortar, from which the shell was fired, was 45 degrees. By means of the line, Mr. Bell and another man worked themselves on shore upon his raft of casks; there were many kinks in the rope, which were with ease cleared by Mr. Bell, in which he was much assisted by his snatch blocks.

A second trial succeeded equally.

The second trial was repeated in a similar manner, and with equal success, the shell falling within a few yards of the former place, the gale of wind was brisk, and the water rough. The direction of the shell was nearly from north to south, and the wind blew nearly north-west.

Inch and half rope thrown 160 yards.

In the third trial, the mortar was elevated to 70 degrees; the rope attached to the shell was an inch and half tarred rope, of which every 50 yards weighed fourteen pounds and a half; the shell of the kind above mentioned. It fell 160 yards from the mortar, and buried itself about two thirds in the ground; the line or rope ran out was about 200 yards, and it required the force of three men to draw the shell out of the ground at that distance.

Two men worked themselves on shore by it.

The grommet, in all these trials, was of white three inch rope; and in all the above trials, by means of the line, two men worked themselves on shore upon the raft: each charge of powder was fifteen ounces.

A grapnel not so good.

A fourth experiment was made by firing, from the same mortar, a grapnel in a wooden case; it did not retain its hold in the ground so well as the shell, but amongst the crevices of rocks, or where the vessel is near shore, will be useful.

Grapnel with an endless rope.

A grapnel of this kind may be fired from a common cannon with an endless rope, running in a pulley or small block

engraving having been omitted at that time, it was thought expedient to insert it in the present publication, with some further particulars then omitted.

Models and Drawings of the whole apparatus are reserved in the Society's Repository, for the inspection of the public.

fixed

fixed thereto, by which a raft may be successively drawn to and from the vessel either by the persons on board the vessel, or those on shore.

Observations made by Lieutenant Bell, upon throwing a Line on Shore in Case of a Ship being stranded.

1st. From the proposed construction of the piece of ordnance, intended to throw the shot and line on shore, I suppose it will be between five and six hundred weight. Weight of the mortar.

The chamber is to contain one pound of powder, and the bore to admit a leaden ball of sixty pounds or upwards; the length of range, or distance, will depend upon the size of the line made use of; I suppose it will carry a deepsea-line between three and four hundred yards distance. Dimensions.

2d. All ships that have iron ballast may use this piece as a part of it, and then there would be only the trifling difference of casting so much of the ballast into the form of the piece; the leaden balls may likewise be used as ballast. May be used as ballast.

3d. I am of opinion, there are various ways, on board of a ship, that the mortar may be placed in a proper position for firing without a carriage expressly made for it; it may be placed upon a coil of rope, or its trunnions rested upon coins, or any thing else, whereby the muzzle can be raised so high, that the groove upon the trunnion appears vertical, as the piece in that position would be elevated nearly 45 degrees. May be used without a carriage.

4th. As I imagine all ships carry deepsea-lines, on that account I made use of it in the experiments at Woolwich; but if it should be thought too short for the distance, any other light line may be added to the length of it. Line.

5th. Supposing a ship's owner to purchase such a piece of ordnance with the leaden balls, and a block carriage; I do not think the whole would amount to more than ten or eleven pounds expense. Cost.

6th. Where a ship is driving or unmanageable near the shore, it would be proper to have the piece loaded, the line reeled upon handspikes or poles, and laid upon the deck ready The line to be coiled on poles.

ready for firing at any time it might be judged necessary. The handspikes or poles the line is reeled upon preserve it in a horizontal form; and they are not to be drawn out until the instant of firing: in this manner the line will deliver itself freely.

Raft. The five water casks should also be prepared in readiness, by lashing them together, and a seaman's chest fixed upon the top of them, having part of its ends or sides cut out, in order to let out such water as may be thrown into it by the surf. I dare undertake to land with such a float upon a lee shore any where upon the coast, when it might be deemed unsafe for a boat to make good its landing.

7th. There is every reason to conclude, that this contrivance would be very useful at all ports of difficult access both at home and abroad, where ships are liable to strike ground before they enter the harbour, as Shields Bar, and other similar situations, when a line might be thrown over the ship, which might probably be the means of saving both lives and property; and moreover, if a ship was driven on shore near such a place, the apparatus might easily be removed to afford assistance; and the whole performance is so exceedingly simple, that any person, once seeing it done, would not want any further instructions.

JOHN BELL.

Woolwich, Aug. 29, 1791.

Some farther Observations made by Lieutenant Bell, upon the Application of the Mortars intended for throwing a Line on Shore, in case of a Ship being stranded.

The mortar would answer for signals, 1st. In trading ships, this piece would answer for making signals of distress, by filling the chamber with powder, and well wadding it, as the report would be heard some miles distance at sea.

or defence. 2d. Such a gun, being accompanied with a few rounds of round and grape shot, would defend a ship much better than a longer gun, against any piratical or other hostile intentions, as, from its shortness, it would be more readily loaded and fired with a larger charge each time.

Not liable to 3d. Accidents from a gun bursting, which may arise from

from an unskilful person loading with too great a proportion of powder, are in this piece effectually guarded against, by the chamber being constructed to contain but one pound of powder, a quantity which is only about one third of the usual charge of a cannon. burst.

4th. From the small size of such a gun and carriage, it might be kept upon deck, without much inconvenience in working the ship, in order to be ready if necessity required; and when the ship is out at sea, it might then be put below. But from the number of dreadful wrecks, which so frequently happen along the coast, it certainly would be prudent to have it always upon deck when within sight of land, and particularly in stormy weather. Not inconvenient to keep on deck.

JOHN BELL.

Woolwich, Sept. 30, 1791.
To C. TAYLOR, M. D. SEC.

Reference to the Engraving of Lieutenant Bell's Method of throwing a Rope on Shore, from a stranded Vessel, Pl. VIII, Fig. 2—7.

a, Represents the mortar on its carriage; *b*, the shell shown within the mortar by dotted lines; *c*, the grommet, or double rope, which connects the shell and line; *d d*, the line to be thrown on shore, now ready wound on the poles or hand-spikes, *p p*, which are to be withdrawn when the mortar is fired. Description of the apparatus.

Fig. 3 Is a separate view of the shell, with the grommet and end of the line attached thereto, explained by the same letters.

Fig. 4 Shows another invention, suggested instead of a shell, and to be fired from a common cannon, in which *e*, is an iron pin; *f*, an iron collar and rope sliding upon it; *g*, an iron ring which turns upon two pins in the collar; *h*, is the grommet or double rope, attached to the ring, to which the line to be thrown on shore is fastened. This plan may be used where people are on shore, to assist when a line is thrown.

Fig. 5 Shows a grapnel, which may also be fired from a

common cannon; the collar slides along it in the same manner as that in fig. 4, to allow the head of the pin to go down to the wadding within the cannon; *i i*, are two pins on which the ring *k* is movable; *l*, the block or pulley fastened to the ring; *m*, the endless or double line running through it.

This method may be used with great advantage, where a ship is stranded near the shore; but where a mortar is on board, the use of the shell and line is the most certain.

Fig. 6 Shows the method of forming a raft, by lashing together with ropes five empty water casks belonging to the ship.

Fig. 7. Represents the raft ready for use; the apparatus *n*, to hold the person upon it, is made from a seaman's chest with holes cut in the sides of it, to allow the person within it firmer hold, and to let out the water that may be thrown into it from the waves; *o o* are two pulleys attached to the ends of the chest, and through which the line is to run; the raft is to be ballasted underneath, to prevent it from upsetting.

The whole apparatus is so arranged as to be enclosed in a small box, as may be seen by a reference to that in the Society's possession.

X.

The Bakerian Lecture, on some new Phenomena of chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalis, and the Exhibition of the new Substances which constitute their Bases; and on the general Nature of alkaline Bodies. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.**

Read November 19, 1807.

I. *Introduction.*

Electricity presumed capable of extending

IN the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a num-

* Philos. Trans. for 1808, Part I, p. 1.

ber

ber of decompositions and chemical changes produced in substances of known composition by electricity; and I ventured to conclude, from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies*.

our knowledge of the elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electro-chemical analysis to bodies, which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results.

This conjecture verified.

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections, particularly those which demonstrate the decomposition and composition of the fixed alkalis, and the production of the new and extraordinary bodies that constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed and often repeated.

Novel processes only described minutely.

II. *On the Methods used for the Decomposition of the fixed Alkalis.*

The researches I had made on the decomposition of acids, and of alkaline and earthy neutral compounds, proved, that the powers of electrical decomposition were proportional to the

The powers of electrical decomposition.

* See Journal, Vol. XVIII, p. 321; and XIX, p. 37.

the strength of the opposite electricities in the circuit, and to the conducting power and degree of concentration of the materials employed.

Aqueous solutions of the alkalis.

In the first attempts that I made on the decomposition of the fixed alkalis, I acted upon aqueous solutions of potash and soda, saturated at common temperatures, by the highest electrical power I could command, and which was produced by a combination of Voltaic batteries belonging to the Royal Institution, containing 24 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hidrogen and oxigen disengaged with the production of much heat and violent effervescence.

Potash in fusion,

The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxigen gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containg potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery of the power of 100 of 6 inches, highly charged; and the connection from the negative side was made by a platina wire.

connected with the positive side.

Appeared to be a powerful conductor.

By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the developement of combustible matter, arose from the point of contact.

Flame emitted.

Connected with the negative side.

When the order was changed, so that the platina spoon was made negative, a vivid and constant light appeared at the opposite point: there was no effect of inflammation round it; but aeriform globules, which inflamed in the atmosphere, rose through the potash.

The platina acted upon.

The platina, as might have been expected, was considerably acted upon: and in the cases when it had been negative, in the highest degree.

The

The alkali was apparently dry in this experiment; and it seemed probable, that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained indeed a number of dark gray metallic particles, but these proved to be derived from the platina.

Inflammable matter from the decomposition of the alkali.

I tried several experiments on the electrization of potash rendered fluid by heat, with the hopes of being able to collect the combustible matter, but without success; and I only attained my object, by employing electricity as the common agent for fusion and decomposition.

Though potash, perfectly dried by ignition, is a nonconductor, yet it is rendered a conductor by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A slight addition of moisture necessary.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Potash exposed to the air a few seconds

and the apparatus in the open air.

Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.

Globules like quicksilver formed.

These globules numerous experiments soon showed to be the substance I was in search of, and a peculiar inflammable principle, the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and

These the inflammable base of potash.

and a substance of the same kind was produced, when pieces of copper, silver, gold, plumbago, or even charcoal were employed for completing the circuit.

Produced in vacuo.

The phenomenon was independent of the presence of air; I found that it took place when the alkali was in the vacuum of an exhausted receiver.

In a glass tube soon dissolved the glass.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires, by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of the alkali, and this substance soon penetrated through the body of the tube.

Soda not so easily decomposed.

Soda, when acted upon in the same manner as potash, exhibited an analogous result; but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of 100 of 6 inches in full activity I obtained good results from pieces of potash weighing from 40 to 70 grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than 15 or 20 grains in weight, and that only when the distance between the wires was about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch.

Its base solid at a lower heat and like silver.

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared to have the lustre of silver.

Soda sometimes exploded.

When the power of 250 was used with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.

III.

III. *Theory of the Decomposition of the fixed Alkalis; their Composition, and Production.*

As in all decompositions of compound substances, which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxigen was produced, and evolved or carried into combination at the positive surface, it was reasonable to conclude, that this substance was generated in a similar manner by the electrical action upon the alkalis; and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case.

Oxigen always produced at the positive surface.

When solid potash, or soda in its conducting state, was included in glass tubes furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved by the most delicate examination to be pure oxigen; and unless an excess of water was present, no gas was evolved from the negative surface.

This the case with the alkalis.

In the synthetical experiments, a perfect coincidence likewise will be found.

Confirmed by synthesis.

I mentioned, that the metallic lustre of the substance from potash immediately became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere, till the whole globule disappeared, and assumed the form of a saturated solution of potash *.

Base of potash soon converted into potash in the air.

When globules were placed in appropriate tubes containing common air or oxigen gas confined by mercury, an ab-

The bases converted into alkali by oxigen.

* Water likewise is decomposed in the process. We shall hereafter see, that the bases of the fixed alkalis act upon this substance with greater energy than any other known bodies. The minute theory of the oxidation of the bases of the alkalis in the free air is this:—oxigen gas is first attracted by them, and alkali formed. This alkali speedily absorbs water. This water is again decomposed. Hence, during the conversion of a globule into alkaline solution, there is a constant and rapid disengagement of small quantities of gas.

Water decomposed in the process.

sorption

sorption of oxigen took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its solution, the process stopped, the interior being defended from the action of the gas.

Base of soda.

With the substance from soda, the appearances and effects were analogous.

Heated in oxigen.

When the substances were strongly heated, confined in given portions of oxigen, a rapid combustion with a brilliant white flame was produced; and the metallic globules were found converted into a white and solid mass, which in the case of the substance from potash was found to be potash, and in the case of that from soda, soda.

Nothing emitted.

Oxigen gas was absorbed in this operation, and nothing emitted which affected the purity of the residual air.

Alkalis produced.

The alkalis produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxigen gas absorbed; and their weights considerably exceeded those of the combustible matters consumed.

The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxigen, and of the respective inflammable substances, which enter into union to form the fixed alkalis, will be given.

Evidence of being a compound of oxigen and a base, the same as with other combustible matters.

It appears then, that in these facts there is the same evidence for the decomposition of potash and soda into oxigen and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxides into oxigen and their respective combustible bases.

In the analytical experiments, no substances capable of decomposition are present but the alkalis and a minute portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated, till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali they come in contact with the heated moistened surface; they cannot be produced from crystallized alkalis, which contain much water; and

and the effect produced by the electrization of ignited potash, which contains no sensible quantity of water, confirms the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalis seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, and the oxigen follows the contrary order*; or, the oxigen being naturally possessed of the negative energy, and the bases of the positive, they do not remain in combination, when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid union is the result; and as in other like cases with the production of fire.—A number of circumstances relating to the agencies of the bases of the alkalis will be immediately stated, and will be found to offer confirmations of these general conclusions.

IV. *On the Properties and Nature of the Basis of Potash.*

After I had detected the bases of the fixed alkalis, I had considerable difficulty to preserve and confine them so as to examine their properties, and submit them to experiments; for, like the *alkahests* imagined by the alchemists, they acted more or less upon almost every body to which they were exposed.

Difficult to preserve and confine the bases.

The fluid substance among all those I have tried, on which I find they have least effect, is recently distilled naphtha.—In this material, when excluded from the air, they remain for many days without considerable changing, and their physical properties may be easily examined in the atmosphere, when they are covered by a thin film of it.

Naphtha least affected by them.

The basis of potash at 60° Fahrenheit, the temperature in which I first examined it, appeared, as I have already mentioned, in small globules possessing the metallic lustre, opacity, and general appearance of mercury; so that when

Base of potash at 60° F. resembles mercury.

* See Bakerian Lecture 1806, p. 28 Phil. Trans. for 1807, or Journal, Vol. XIX, p. 41.

a glo-

a globule of mercury was placed near a globule of the peculiar substance, it was not possible to detect a difference by the eye.

But its fluidity imperfect below 100°.

At 60° Fahrenheit it is however only imperfectly fluid, for it does not readily run into a globule, when its shape is altered; at 70° it becomes more fluid; and at 100° its fluidity is perfect, so that different globules may be easily made to run into one.

At 50° soft and malleable.

At 50° Fahrenheit it becomes a soft and malleable solid, which has the lustre of polished silver; and at about the freezing point of water it becomes harder and

At 32° brittle.

brittle, and when broken in fragments, exhibits a crystallized texture, which in the microscope seems composed of beautiful facets of a perfect whiteness and high metallic splendour.

Distilled without change.

To be converted into vapour, it requires a temperature approaching that of the red heat; and when the experiment is conducted under proper circumstances, it is found unaltered after distillation.

A perfect conductor of electricity,

It is a perfect conductor of electricity. When a spark from the Voltaic battery of 100 of 6 inches is taken upon a large globule in the atmosphere, the light is green, and combustion takes place at the point of contact only. When a small globule is used, it is completely dissipated with explosion, accompanied by a most vivid flame, into alkaline fumes.

and of heat.

It is an excellent conductor of heat.

Its specific gravity

Resembling the metals in all these sensible properties, it is however remarkably different from any of them in specific gravity; I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was ·861; and it did not sink in double distilled naphtha, the specific gravity of which was about ·77°, that of water being considered as 1. The small quantities in which it is produced by the highest electrical powers, rendered it very difficult to determine this quality with minute precision. I endeavoured to gain approximations on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury. I used the very delicate balance of the Royal Institution, which when loaded with the quantities I employed, and of which the mercury never exceeded ten

ten grains, is sensible at least to the $\frac{1}{1000}$ of a grain. Taking the mean of 4 experiments, conducted with great care, its specific gravity at 62° Fahrenheit is to that of mercury as 10 to 223, which gives a proportion to that of water nearly as 6 to 10; so that it is the lightest fluid body known. In its solid form it is a little heavier, but even in this state, when cooled to 40° Fahrenheit, it swims in the double distilled naphtha.

about ·6 when fluid, and less than ·77 when solid.

The chemical relations of the basis of potash are still more extraordinary than its physical ones.

Its chemical relations more extraordinary.

I have already mentioned its alkalization and combustion in oxigen gas.—It combines with oxigen slowly, and without flame, at all temperatures that I have tried below that of its vaporization.—But at this temperature combustion takes place, and the light is of a brilliant whiteness and the heat intense. When heated slowly in a quantity of oxigen gas not sufficient for its complete conversion into potash, and at a temperature inadequate to its inflammation, 400° Fahrenheit, for instance, its tint changes to that of a red brown, and when the heat is withdrawn, all the oxigen is found to be absorbed, and a solid is formed of a grayish colour, which partly consists of potash and partly of the basis of potash in a lower degree of oxigenation,—and which becomes potash by being exposed to water, or by being again heated in fresh quantities of air.

To oxigen.

The substance consisting of the basis of potash combined with an under proportion of oxigen may likewise be formed by fusing dry potash and its basis together under proper circumstances.—The basis rapidly loses its metallic splendour; the two substances unite into a compound of a red brown colour when fluid, and of a dark gray hue when solid; and this compound soon absorbs its full proportion of oxigen when exposed to air, and is wholly converted into potash.

And the same body is often formed in the analytical experiments when the action of the electricity is intense, and the potash much heated.

The basis of potash when introduced into oximuriatic acid gas burns spontaneously with a bright red light; and a white salt, proving to be muriate of potash, is formed.

Burns in oximuriatic acid.

When

Dissolves in hot hidrogen gas.

When a globule is heated in hidrogen at a degree below its point of vaporization, it seems to dissolve in it, for the globule diminishes in volume, and the gas explodes with alkaline fumes and bright light, when suffered to pass into the air; but by cooling, this spontaneous detonating property is destroyed, and the basis is either wholly or principally deposited.

Action of water.

The action of the basis of potash on water exposed to the atmosphere is connected with some beautiful phenomena. When it is thrown upon water, or when it is brought into contact with a drop of water at common temperatures, it decomposes it with great violence, an instantaneous explosion is produced with brilliant flame, and a solution of pure potash is the result.

White ring of smoke.

In experiments of this kind, an appearance often occurs similar to that produced by the combustion of phosphuretted hidrogen; a white ring of smoke, which gradually extends as it rises into the air.

Action of water when air is excluded.

When water is made to act upon the basis of potash out of the contact of air, and preserved by means of a glass tube under naphtha, the decomposition is violent; and there is much heat and noise, but no luminous appearance, and the gas evolved when examined in the mercurial or water pneumatic apparatus is found to be pure hidrogen.

Ice.

When a globule of the basis of potash is placed upon ice, it instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash.

Action of water on it in the open air explained.

The theory of the action of the basis of potash upon water exposed to the atmosphere, though complicated changes occur, is far from being obscure. The phenomena seem to depend on the strong attractions of the basis for oxigen, and of the potash formed for water. The heat, which arises from two causes, decomposition and combination, is sufficiently intense to produce the inflammation. Water is a bad conductor of heat; the globule swims exposed to air; a part of it, there is the greatest reason to believe, is dissolved by the heated nascent hidrogen; and this substance, being capable of spontaneous inflammation, explodes, and communicates

communicates the effect of combustion to any of the basis that may be yet uncombined.

Out of the contact of air.

When a globule confined out of the contact of air is acted upon by water, the theory of decomposition is very simple, the heat produced is rapidly carried off, so that there is no ignition; and a high temperature being requisite for the solution of the basis in hidrogen, this combination probably does not take place, or at least it can have a momentary existence only.

Moistened turmeric paper.

The production of alkali in the decomposition of water by the basis of potash is demonstrated in a very simple and satisfactory manner by dropping a globule of it upon moistened paper tinged with turmeric. At the moment that the globule comes into contact with the water, it burns, and moves rapidly upon the paper, as if in search of moisture, leaving behind it a deep reddish brown trace, and acting upon the paper precisely as dry caustic potash.

Decomposes the small quantity of water in purified ether and alcohol.

So strong is the attraction of the basis of potash for oxigen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified.

Ether.

In ether this decomposition is connected with an instructive result. Potash is insoluble in this fluid; and when the basis of potash is thrown into it, oxigen is furnished to it, and hidrogen gas disengaged, and the alkali as it forms renders the ether white and turbid.

In both these inflammable compounds the energy of its action is proportional to the quantity of water they contain, and hidrogen and potash are the constant result.

Mineral acids.

The basis of potash when, thrown into solutions of the mineral acids, inflames and burns on the surface. When it is plunged by proper means beneath the surface enveloped in potash, surrounded by naphtha, it acts upon the oxigen with the greatest intensity, and all its effects are such as may be explained from its strong affinity for this substance. In sulphuric acid a white saline substance with a yellow coating, which is probably sulphate of potash surrounded by sulphur, and a gas which has the smell of sulphurous acid, and which probably is a mixture of that substance with

with hidrogen gas, are formed. In nitrous acid, nitrous gas is disengaged, and nitrate of potash formed.

Simple inflammables.

The basis of potash readily combines with the simple inflammable solids, and with the metals; with phosphorus and sulphur it forms compounds similar to the metallic phosphurets and sulphurets.

Phosphorus.

When it is brought into contact with a piece of phosphorus, and pressed upon, there is a considerable action: they become fluid together, burn, and produce phosphate of potash. When the experiment is made upon naphtha, their combination takes place without the liberation of any elastic matter, and they form a compound, which has a considerably higher point of fusion than its two constituents, and which remains a soft solid in boiling naphtha. In its appearance it perfectly agrees with a metallic phosphuret, it is of the colour of lead, and when spread out, has a lustre similar to polished lead. When exposed to air at common temperatures, it slowly combines with oxigen, and becomes phosphate of potash. When heated upon a plate of platina, fumes exhale from it, and it does not burn, till it attains the temperature of the rapid combustion of the basis of potash.

Sulphur.

When the basis of potash is brought into contact with sulphur in fusion, in tubes filled with the vapour of naphtha, they combine rapidly with evolution of heat and light, and a gray substance, in appearance like artificial sulphuret of iron, is formed, which, if kept in fusion, rapidly dissolves the glass, and becomes bright brown. When this experiment is made in a glass tube hermetically sealed, no gas is liberated, if the tube is opened under mercury; but when it is made in a tube connected with a mercurial apparatus, a small quantity of sulphuretted hidrogen is evolved, so that the phenomena are similar to those produced by the union of sulphur with the metals in which sulphuretted hidrogen is likewise disengaged, except that the ignition is stronger*.

When

Sulphur contains hidrogen.

* The existence of hidrogen in sulphur is rendered very probable by the ingenious researches of Mr. Berthollet, Jun Annales de Chimie, Fevrier 1807, p. 143. (See Journal vol. XVIII, p. 50.) This fact is almost demonstrated by an experiment, which I saw made by W. Clayfield, Esq , at Bristol, in 1799. Copper filings and powdered sulphur, in weight in

When the union is effected in the atmosphere, a great inflammation takes place, and sulphuret of potash is formed. The sulphuretted basis likewise gradually becomes oxigenated by exposure to the air, and is finally converted into sulphate.

Mercury.

The new substance produces some extraordinary and beautiful results with mercury. When one part of it is added to 8 or 10 parts of mercury in volume 160° Fahrenheit, they instantly unite and form a substance exactly like mercury in colour, but which seems to have less coherence, for small portions of it appear as flattened spheres. When a globule is made to touch a globule of mercury about twice as large, they combine with considerable heat; the compound is fluid at the temperature of its formation; but when cool it appears as a solid metal, similar in colour to silver. If the quantity of the basis of potash is still farther increased, so as to be about $\frac{1}{36}$th the weight of the mercury, the amalgam increases in hardness, and becomes brittle. The solid amalgam, in which the basis is in the smallest proportion, seems to consist of about 1 part in weight of basis and 70 parts of mercury, and is very soft and malleable.

Separates unaltered from the potash formed.

When these compounds are exposed to air, they rapidly absorb oxigen; potash which deliquesces is formed; and in a few minutes the mercury is found pure and unaltered.

The amalgam decomposes water,

When a globule of the amalgam is thrown into water, it rapidly decomposes it with a hissing noise; potash is formed, pure hidrogen disengaged, and the mercury remains free.

and dissolves all metals.

The fluid amalgam of mercury and this substance dissolves all the metals I have exposed to it; and in this state of union mercury acts on iron and platina.

in the proportion of three to one, rendered very dry, were heated together in a retort, connected with a mercurial pneumatic apparatus. At the moment of combination a quantity of elastic fluid was liberated amounting to 9 or 10 times the volume of the materials employed, and which consisted of sulphuretted hidrogen mixed with sulphurous acid. The first mentioned product, there is every reason to believe, must be referred to the sulphur, the last probably to the copper, which it is easy to conceive may have become slightly and superficially oxidated during the processes of filing and drying by heat.

When

The base of potash unites with gold, silver, and copper, and

When the basis of potash is heated with gold, or silver, or copper, in a close vessel of pure glass, it rapidly acts upon them; and when the compounds are thrown into water, this fluid is decomposed, potash formed, and the metals appear to be separated unaltered.

renders fusible metal less fusible.

The basis of potash combines with fusible metal, and forms an alloy with it, which has a higher point of fusion than the fusible metal.

Its action on oily compounds.

The action of the basis of potash upon the inflammable oily compound bodies confirms the other facts of the strength of its attraction for oxigen.

On naphtha.

On naphtha colourless and recently distilled, as I have already said, it has very little power of action; but in naphtha that has been exposed to the air it soon oxidates, and alkali is formed, which unites with the naphtha into a brown soap, that collects round the globule.

On concrete oils.

On the concrete oils (tallow, spermaceti, wax, for instance), when heated, it acts slowly, coaly matter is deposited, a little gas* is evolved, and a soap is formed; but in these cases it is necessary that a large quantity of the oil be employed.

On fluid fixed oils.

On the fluid fixed oils it produces the same effects, but more slowly.

On volatile oils.

By heat likewise it rapidly decomposes the volatile oils; alkali is formed, a small quantity of gas is evolved, and charcoal is deposited.

* When a globule of the basis of potash is introduced into any of the fixed oils heated, the first product is pure hidrogen, which arises from the decomposition of the water absorbed by the crust of potash during the exposure to the atmosphere. The gas evolved, when the globule is freed from this crust, I have found to be carbonated hidrogen requiring more than an equal bulk of oxigen gas for its complete saturation by explosion. I have made a great number of experiments, which it would be foreign to the object of this lecture to give in minute detail, on the agencies of the basis of potash on the oils. Some anomalies occurred which led to the inquiry, and the result was perfectly conclusive. Olive oil, oil of turpentine, and naphtha when decomposed by heat, exhibited as products different proportions of charcoal, heavy inflammable gas, empyreumatic oily matter, and water, so that the existence of oxigen in them was fully proved; and accurate indications of the proportions of their elements might be gained by their decomposition by the basis of potash. Naphtha of all furnished least water and carbonic acid, and oil of turpentine the most.

When

When the basis of potash is thrown into camphor in fusion, the camphor soon becomes blackened, no gas is liberated in the process of decomposition, and a saponaceous compound is formed; which seems to show, that camphor contains more oxigen than the volatile oils. On camphor.

The basis of potash readily reduces metallic oxides when heated in contact with them. When a small quantity of the oxide of iron was heated with it to a temperature approaching its point of distillation, there was a vivid action; alkali and gray metallic particles, which dissolved with effervescence in muriatic acid, appeared. The oxides of lead and the oxides of tin were revived still more rapidly; and when the basis of potash was in excess, an alloy was formed with the revived metal. On metallic oxides.

In consequence of this property, the basis of potash readily decomposes flint glass and green glass, by a gentle heat; alkali is immediately formed by oxigen from the oxides, which dissolves the glass, and a new surface is soon exposed to the agent. On glass.

At a red heat, even the purest glass is altered by the basis of potash: the oxigen in the alkali of the glass seems to be divided between the two bases, the basis of potash and the alkaline basis in the glass, and oxides, in the first degree of oxigenation, are the result. When the basis of potash is heated in tubes made of plate glass filled with the vapour of naphtha, it first acts upon the small quantity of the oxides of cobalt and manganese in the interior surface of the glass, and a portion of alkali is formed. As the heat approaches to redness, it begins to rise in vapour, and condenses in the colder parts of the tube; but at the point where the heat is strongest, a part of the vapour seems to penetrate the glass, rendering it of a deep red brown colour; and by repeatedly distilling and heating the substance in a close tube of this kind, it finally loses its metallic form, and a thick brown crust, which slowly decomposes water, and which combines with oxigen when exposed to air, forming alkali, lines the interior of the tube, and in many parts is found penetrating through its substance*. Its action on glass explained.

In

* This is the obvious explanation in the present state of our knowledge; Perhaps the si-

In my first experiments on the distillation of the basis of potash, I had great difficulty in accounting for these phenomena; but the knowledge of the substance it forms in its first degree of union with oxigen afforded a satisfactory explanation.

V. *On the Properties and Nature of the Basis of Soda.*

Basis of soda.

The basis of soda, as I have already mentioned, is a solid at common temperatures. It is white, opaque, and when examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is much softer than any of the common metallic substances. When pressed upon by a platina blade, with a small force, it spreads into thin leaves, and a globule of the $\frac{1}{10}$th, or $\frac{1}{12}$th of an inch in diameter is easily spread over a surface of a quarter of an inch*, and this property does not diminish when it is cooled to 32° Fahrenheit.

Conducts heat and electricity.

It conducts electricity and heat in a similar manner to the basis of potash; and small globules of it inflame by the voltaic electrical spark, and burn with bright explosions.

Specific gravity ·9348.

Its specific gravity is less than that of water. It swims in oil of sassafras of 1·096, water being 1, and sinks in naphtha of specific gravity ·861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions till I had composed a fluid, in which it remained at rest above or below; and this fluid consisted of nearly twelve parts naphtha, and five of oil of sassafras, which gives a specific gravity to that of water nearly as nine to ten, or more accurately as ·9348 to 1.

Perfectly fluid at 180°.

The basis of soda has a much higher point of fusion than the basis of potash; its parts begin to lose their cohesion at

lex of the glass altered.

ledge; but it is more than probable, that the silex of the glass likewise suffers some change, and probably decomposition. This subject I hope to be able to resume on another occasion.

Welds at common temperatures.

* Globules may be easily made to adhere and form one mass by strong pressure: so that the property of welding, which belongs to iron and platina at a white heat only, is possessed by this substance at common temperatures.

about

about 120° Fahrenheit, and it is a perfect fluid at about 180°, so that it readily fuses under boiling naphtha.

I have not yet been able to ascertain at what degree of heat it is volatile; but it remains fixed in a state of ignition at the point of fusion of plate glass. Not easily volatilized.

The chemical phenomena produced by the basis of soda are analogous to those produced by the basis of potash; but with such characteristic differences as might be well expected. Its properties analogous to those of the base of potash.

When the basis of soda is exposed to the atmosphere, it immediately tarnishes, and by degrees becomes covered with a white crust, which deliquesces much more slowly than the substance which forms on the basis of potash. It proves, on minute examination, to be pure soda. Action of the air on it.

The basis of soda combines with oxigen slowly, and without any luminous appearance, at all common temperatures; and when heated, this combination becomes more rapid; but no light is emitted, till it has acquired a temperature near that of ignition. Of oxigen.

The flame that it produces in oxigen gas is white, and it sends forth bright sparks, occasioning a very beautiful effect; in common air, it burns with light of the colour of that produced during the combustion of charcoal, but much brighter.

The basis of soda when heated in hidrogen, seemed to have no action upon it. When introduced into oximuriatic acid gas, it burnt vividly with numerous scintillations of a bright red colour. Saline matter was formed in this combustion, which, as might have been expected, proved to be muriate of soda. Of hidrogen. Of oximuriatic acid gas.

Its operation upon water offers most satisfactory evidence of its nature. When thrown upon this fluid, it produces a violent effervescence, with a loud hissing noise; it combines with the oxigen of the water to form soda, which is dissolved, and its hidrogen is disengaged. In this operation there is no luminous appearance; and it seems probable, that even in the nascent state hidrogen is capable of combining with it*. Of water.

When the basis of soda is thrown into hot water, the de- Of hot water.

* The more volatile metals only seem capable of uniting with hidrogen; a circumstance presenting an analogy.

composition is more violent, and in this case a few scintillations are generally observed at the surface of the fluid; but this is owing to small particles of the basis, which are thrown out of the water sufficiently heated, to burn in passing through the atmosphere. When, however, a globule is brought into contact with a small particle of water, or with moistened paper, the heat produced (there being no medium to carry it off rapidly) is usually sufficient for the accension of the basis.

Of alcohol and ether.

The basis of soda acts upon alcohol and ether precisely in a similar manner with the basis of potash. The water that they contain is decomposed; soda is rapidly formed, and hidrogen disengaged.

Of acids.

The basis of soda, when thrown upon the strong acids, acts upon them with great energy. When nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acid, there is much heat generated, but no light.

When plunged, by proper means, beneath the surface of the acids, it is rapidly oxigenated; soda is produced, and the other educts are similar to those generated by the action of the basis of potash.

Of oils and naphtha.

With respect to the fixed and volatile oils and naphtha in their different states, there is a perfect coincidence between the effects of the two new substances, except in the difference of the appearances of the saponaceous compounds formed: those produced by the oxidation and combination of the basis of soda being of a darker colour, and apparently less soluble.

Of oxigen.

The basis of soda, in its degrees of oxidation, has precisely similar habits with the basis of potash.

When it is fused with dry soda, in certain quantities, there is a division of oxigen between the alkali and the base; and a deep brown fluid is produced, which becomes a dark gray solid on cooling, and which attracts oxigen from the air, or which decomposes water, and becomes soda.

The same body is often formed in the analytical processes of decomposition, and it is generated when the basis of soda is fused in tubes of the purest plate glass.

Of inflammables.

There is scarcely any difference in the visible phenomena of

of the agencies of the basis of soda, and that of potash, on sulphur, phosphorus, and the metals.

It combines with sulphur in close vessels filled with the vapour of naphtha with great vividness, with light, heat, and often with explosion from the vaporization of a portion of sulphur, and the disengagement of sulphuretted hidrogen gas. The sulphuretted basis of soda is of a deep gray colour. Of sulphur.

The phosphuret has the appearance of lead, and forms phosphate of soda by exposure to air, or by combustion. Of phosphorus.

The basis of soda in the quantity of $\frac{1}{40}$ renders mercury a fixed solid of the colour of silver, and the combination is attended with a considerable degree of heat. Of mercury.

It makes an alloy with tin, without changing its colour, and it acts upon lead and gold when heated. I have not examined its habitudes with any other metals; but in its state of alloy it is soon converted into soda by exposure to air, or by the action of water, which it decomposes with the evolution of hidrogen. Of tin, lead, and gold.

The amalgam of mercury and the basis of soda seems to form triple compounds with other metals. I have tried iron and platina, which I am inclined to believe remain in combination with the mercury, when it is deprived of the new substance by exposure to air. Its amalgam with other metals

The amalgam of the basis of soda and mercury likewise combines with sulphur, and forms a triple compound of a dark gray colour. and with sulphur.

VI. *On the Proportions of the peculiar Bases and Oxigen in Potash and Soda.*

The facility of combustion of the bases of the alkalis, and the readiness with which they decomposed water, offered means fully adequate for determining the proportions of their ponderable constituent parts. Proportions of the bases to oxigen to form alkalis.

I shall mention the general methods of the experiments, and the results obtained by the different series, which approach as near to each other as can be expected in operations performed on such small quantities of materials.

For the process in oxigen gas I employed glass tubes containing small trays made of thin leaves of silver, or other noble metals, on which the substance to be burnt, after being Process to determine these.

being accurately weighed or compared with a globule of mercury equal in size*, was placed: the tube was small at one end, curved, and brought to a fine point, but suffered to remain open; and the other end was fitted to a tube communicating with a gasometer, from which the oxigen gas was introduced, for neither water nor mercury could be used for filling the apparatus. The oxigen gas was carried through the tube, till it was found that the whole of the common air was expelled. The degree of its purity was ascertained by suffering a small quantity to pass into the mercurial apparatus. The lower orifice was then hermetically sealed by a spirit lamp, and the upper part drawn out and finally closed, when the aperture was so small, as to render the temperature employed incapable of materially influencing the volume of the gas; and when the whole arrangement was made, the combination was effected by applying heat to the glass in contact with the metallic tray.

Difficulties.

In performing these experiments many difficulties occurred. When the flame of the lamp was immediately brought to play upon the glass, the combustion was very vivid, so as sometimes to break the tube; and the alkali generated partly rose in white fumes, which were deposited upon the glass.

When the temperature was slowly raised, the bases of the alkalis acted upon the metallic tray and formed alloys, and in this state it was very difficult to combine them with their full propotion of oxigen; glass alone could not be employed on account of its decomposition by the alkaline bases; and porcelain is so bad a conductor of heat, that it was not possible to raise it to the point required for the process, without softening the glass.

In all cases the globules of the alkaline bases were carefully freed from naphtha before they were introduced; of course a slight crust of alkali was formed before the com-

* When the globules were very small, the comparison with mercury, which may be quickly made by means of a micrometer, was generally employed as the means of ascertaining the weight: for in this case the globule could be immediately introduced into the tube, and the weight of mercury ascertained at leisure.

bustion,

bustion, but this could not materially affect the result; and when such a precaution was not used, an explosion generally took place from the vaporization and decomposition of the film of naphtha surrounding the globule.

After the combustion, the absorption of gas was ascertained, by opening the lower point of the tube under water or mercury. In some cases the purity of the residual air was ascertained, in others the alkali formed in the tray was weighed.

Two synthetical experiments on potash selected.

From several experiments on the synthesis of potash by combustion, I shall select two, which were made with every possible attention to accuracy, and under favourable circumstances, for a mean result.

1st experiment.

In the first experiment 0·12 of a grain of the basis were employed. The combustion was made upon platina, and was rapid and complete; and the basis appeared to be perfectly saturated, as no disengagement of hidrogen took place, when the platina tray was thrown into water. The oxigen gas absorbed equalled in volume 190 grain measures of quicksilver; barometer being at 29·6 inches, thermometer 62° Fahrenheit; and this reduced to a temperature of 60° Fahrenheit, and under a pressure equal to that indicated by 30 inches *, would become 186·67 measures, the weight of which would be about ·0184 grain troy †; but ·0184 : ·1384 : : 13·29 : 100; and according to this estimation 100 parts of potash will consist of 86·7 basis, and 13·3 oxigen nearly.

2d experiment.

In the second experiment ·07 grains of the basis absorbed at temperature 63° of Fahrenheit, and under pressure equal to 30·1 barometer inches, a quantity of oxigen

* In the correction for temperature, the estimations of Dalton and Gay Lussac are taken, which make gasses expand about $\frac{1}{480}$ of the primitive volume for every degree of Fahrenheit.

† From experiments that I made in 1799, on the specific gravity of oxigen gas, it would appear, that its weight is to that of water as 1 to 748, and to that of quicksilver as 1 to 10142. *Researches Chem.* and *Phil.* p. 9; and with this estimation, that deducible from the late accurate researches of Messrs. Allen and Pepys on the Combustion of the Diamond almost precisely agrees, Phil. Trans. 1807, page 275; or our Journal, vol. XIX, p. 223.

equal

equal in volume to 121 grain measures of mercury, and the proper corrections being made as in the former case, this gas would weigh ·01189 of a grain.

Mean 86·1 base to 13·9 oxigen.

But ·07 + ·01189 = ·08189 : 07 : : 100 : 85·48 nearly, 100 parts of potash will consist of 85·5 of basis and 14·5 of oxigen nearly. And the mean of the two experiments will be 86·1 of basis to 13·9 of oxigen for 100 parts.

Experiment with soda.

In the most accurate experiment that I made on the combustion of the basis of soda ·08 parts of the basis absorbed a quantity of oxigen equal to 206 grain measures of mercury; the thermometer being at 56° Fahrenheit, and the barometer at 29·4; and this quantity, the corrections being made as before for the mean temperature and pressure, equals about ·02 grains of oxigen.

80 base to 20 oxigen.

And as ·08 + ·02 = ·10 : ·08, : : 100 : 80, 100 parts of soda, according to this estimation, will consist of 80 basis to 20 of oxigen.

Increase of weight indicated more oxigen

In all cases of slow combustion, in which the alkalis were not carried out of the tray, I found a considerable increase of weight; but as it was impossible to weigh them except in the atmosphere, the moisture attracted rendered the results doubtful; and the proportions from the weight of the oxigen absorbed are more to be depended on. In the experiments in which the processes of weighing were most speedily performed, and in which no alkali adhered to the tube, the basis of potash gained nearly 2 parts for 10, and that of soda between 3 and 4 parts.

but less to be depended on.

Decomposition of water by the bases.

The results of the decomposition of water by the bases of the alkalis were much more readily and perfectly obtained than those of their combustion.

Amalgam of base of potash employed.

To check the rapidity of the process, and, in the case of potash, to prevent any of the basis from being dissolved, I employed the amalgams with mercury. I used a known weight of the bases, and made the amalgams under naphtha, using about two parts of mercury in volume to one of basis.

In the first instances I placed the amalgams under tubes filled with naphtha, and inverted in glasses of naphtha, and slowly admitted water to the amalgam at the bottom of the glass; but this precaution I soon found unnecessary, for the action

action of the water was not so intense, but that the hidrogen gas could be wholly collected.

I shall give an account of the most accurate experiments made on the decomposition of water by the bases of potash and soda.

In an experiment on the basis of potash conducted with every attention that I could pay to the minutiæ of the operations, hidrogen gas, equal in volume to 298 grains of mercury, was disengaged by the action of ·08 of a grain of the basis of potash, which had been amalgamated with about 3 grains of mercury. The thermometer at the end of the process indicated a temperature of 56° Fahrenheit, and the barometer an atmospheric pressure equal to 29·6 inches. Experiment.

Now this quantity of hidrogen* would require for its combustion a volume of oxigen gas about equal to that occupied by 154·9 grains of mercury, which gives the weight of oxigen required to saturate the ·08 of a grain of the basis of potash at the mean temperature and pressure nearly ·0151 of a grains. And $\cdot 08 + \cdot 0151 = \cdot 0951 : \cdot 08 :: 100 : 84 \cdot 1$ nearly.

And according to these indications 100 parts of potash consist of about 84 basis and 16 oxigen. Gave 84 base to 16 oxigen.

In an experiment on the decomposition of water by the basis of soda, the mercury in the barometer standing at 30·4 inches, and in the thermometer at 52° Fahrenheit, the volume of hidrogen gas evolved by the action of ·054 of a grain of basis equalled that of 326 grains of quicksilver. Now this at the mean temperature and pressure would require for its conversion into water, ·0172 of oxigen, and $\cdot 054 + \cdot 0172 = \cdot 0712 : \cdot 054 :: 100 : 76$ nearly; and according to these indications, 100 parts of soda consist of nearly 76 basis, and 24 oxigen. Experiment with base of soda. Gave 76 base to 24 oxigen.

In another experiment made with very great care, ·052 of the basis of soda were used; the mercury in the barometer was at 29·9 inches, and that in the thermometer at 58° Fahrenheit. The volume of hidrogen evolved was equal to that of 302 grains of mercury; which would demand for Another experiment

* *Researches Chem.* and *Phil.* page 287.

its

its saturation by combustion at the mean temperature and pressure ·01549 of a grain of oxigen; and 100 parts of soda, according to this proportion, would consist nearly of 77 basis, and 23 oxigen.

gave 77 base to 23 oxigen.

Several other experiments made.

The experiments, which have been just detailed, are those in which the largest quantities of materials were employed; I have compared their results, however, with the results of several others, in which the decomposition of water was performed with great care, but in which the proportion of the bases was still more minute: the largest quantity of oxigen indicated by these experiments was, for potash 17, and for soda 26 parts in 100, and the smallest 13, and 19; and comparing all the estimations, it will probably be a good approximation to the truth, to consider potash as composed of about 6 parts basis and 1 of oxigen; and soda, as consisting of 7 basis and 2 oxigen.

From a comparison of the whole 6 base to 1 oxigen for potash and 7 base to 2 oxigen for soda probably near the truth.

(To be concluded in our next.)

XI.

Remarks on Iron Spar: by Mr. Bergman*.

Mr. Berthier found no perceptible portion of lime in iron spar.

Mr. Haüy, having been informed in a letter from Mr. Hassenfratz, that Mr. Berthier, in his analysis of iron spar, had found merely inperceptible traces of the presence of lime, sent to the laboratory of investigation belonging to the Museum two pieces of this ore, one of which was black, the other white, both regularly crystallized and free from any gangue, that they might be examined for the existence of lime. The following are the results of this preliminary examination.

Black iron spar.

Component parts of black iron spar, according to the author:

Iron, at a minimum	62
Carbonic acid united with the iron	16·9
Carbonate of lime	5
Water of crystallization	16·1
	100

* Journal des Mines, No. III, p. 241.

White

White iron spar.

Iron, at a minimum	25
Carbonic acid united with the iron	6·8
Carbonate of lime	48
Water of crystallization	17·2
Pyrites	3
	100

and of white iron spar.

Mr Drappier's results very different.

After the publication of Mr. Drappier on the same subject, whose results were so different from mine, I examined anew the products, which I had carefully preserved: and accordingly I treated the 48 parts of carbonate of lime, found in the white iron spar, with weak sulphuric acid. A very brisk effervescence took place, and a very bulky magma was formed, which had all the characters of sulphate of lime. This matter, having been heated with the usual precautions to expel the moisture, was slightly calcined to drive off the excess of acid; diluted with a very small quantity of water; and filtered. The liquor had a bitter taste similar to that of sulphate of magnesia, but slightly metallic. The residuum, separated from the filter, and calcined, was perfectly white and insipid. It weighed 37 parts. If we admit 32 parts of lime in 100 of crystallized sulphate, there will be 23 in the 57 calcined: and if there be 44 parts of carbonic acid in 100 of carbonate, there must have been only 41 per cent of carbonate of lime, instead of 48 per cent mentioned above.

Supposed carbonate of lime examined.

7 parts of it not lime,

The liquor mentioned above was left to evaporate slowly in the open air. After a few days the whole was crystallized into a white salt, that weighed 26 parts. The solution of this salt in water was very bitter, and still retained its metallic taste. On caustic potash being added, a bulky white precipitate was formed, which had the appearance of magnesia. When separated, dried, and calcined, it was of a light violet colour, owing to the presence of oxide of manganese, and weighed 5 parts. These being added to the 41 of carbonate of lime give but 2 of loss, which may be ascribed to carbonic acid belonging to the magnesia. Thus we must admit 7 per cent of carbonate of magnesia, the quantity of manganese being but very small.

but carbonate of magnesia,

The magnesia coloured with manganese was treated with radical

with a minute

portion of manganese.

radical vinegar a little diluted, and the whole was dissolved, except some traces of black oxide of manganese. The solution was slightly coloured. On heating it, it became colourless; and though the precipitate was a little increased by this ebullition, it could not be weighed on account of the smallness of its quantity.

The supposed iron oxide contained 4 parts of manganese.

As the iron might contain manganese, it was calcined with caustic potash, which thus acquired a very deep green colour. The calcination with potash was repeated, till the intensity of the colour was so far diminished, as to render it almost certain, that the whole of the manganese was separated. The alkaline liquor being saturated by an acid, the manganese was precipitated by ammonia. It weighed 4 parts. The true results therefore of the analysis of white iron spar are

Real component parts of white iron spar.

Iron	20·
Manganese	4·5
Carbonic acid united with the iron	6·8
Carbonate of lime	41
Carbonate of magnesia	7
Loss and water of crystallization	17·2
Pyrites	3
	100

Examination of the products of analysis of black iron spar.

Preceding analysis of black iron spar examined.

The five parts of carbonate of lime mentioned above, being treated in the same manner, were found to contain merely an atom of lime, the quantity of which was too small to be estimated. They consisted almost wholly of magnesia, with a little manganese. The iron too contained a perceptible quantity of manganese, which could not be separated from it completely but by repeated calcination with caustic potash.

The following alterations therefore must be made in the results of the analysis of the black iron spar.

Its real component parts.

Oxide of iron and of manganese	64
Carbonic acid united with the 2 metals	16·9
Carbonate of magnesia	3
Loss and water of crystallization	16·1
	100

XII.

XII.

Analysis of a Urinary Calculus: by Professor Wurzer *.

FOR the stone I have now analysed I am indebted to Mr. Michaelis, who extracted it from a patient by the operation.

It was nearly oval, but a little flattened: brown exteriorly, and of a yellowish white within. It weighed exactly 870 grains German weight [834 grs. Eng.]. Its specific gravity was 1·572. Its surface was irregular, and a little rough. It was of the consistence of hard chalk, was without a nucleus, and composed of layers.

Physical characters of the stone.

1. I macerated 300 grains of this concretion, previously powdered, in distilled water at the temperature of 12° R. [59° F.] for two days. Having filtered the liquor, it was without colour; and neither afforded any precipitate, nor was perceptibly changed, by nitrate of mercury, nitrate of silver, muriate of barytes, barytes-water, lime-water, oxalic acid, potash, or ammonia. It is evident therefore, that the distilled water had taken up none of the constituent parts of this urinary concretion.

Chemical examination.

Water took up nothing.

The powder when dried weighed as much as at first.

2. This powder I left for two days in muriatic acid of the specific gravity of 1·181, at a temperature of 15° R. [65·75° F.], and then added to it distilled water. After filtering, I dried the residuum thoroughly, which then weighed 248 grains, and was of a reddish brown colour.

Muriatic acid took up

3. The filtered liquor, precipitated by lime-water, afforded a powder, which when collected and examined was found to be phosphate of lime. It weighed 52 grains.

phosphate of lime.

4. The 248 grains that remained from the second experiment were put into a solution of potash a little diluted, and left in it for two days at a temperature of 18° R. [72·5° F.]. I then filtered off the liquor, from which acetous acid threw down a precipitate weighing 230 grains. This, carefully examined, consisted of 226 grains of uric acid, easily distinguishable by its properties and characteristics, and about 4 grains of animal matter.

Potash dissolved uric acid, & some animal matter.

* Annales de Chimie, vol LX, p. 310.

5. What

Undissolved animal matter burned

5. What remained on the filter weighed 18 grains. This I heated to incandescence in a silver crucible. During this process a very disagreeable fetid smell was emitted, resembling that of horn or hair burning. The residuum weighed scarcely 3 grains.

left 3 grains

6. These 3 grains were not soluble in sulphuric, nitric, or muriatic acid, even when heated with them in succession to ebullition.

which were silex

7. I then mixed them with four times their weight of potash, and melted the mixture in a suitable heat. The whole dissolved in water, and I precipitated pure silex by adding an acid in excess.

This a rare occurrence.

This earth was found but twice by Messrs. Fourcroy and Vauquelin in urinary calculi, though they analysed a very great number; which induced me to repeat my operation with the 570 grains I had reserved. As I again found silex, and in a similar proportion, in these, I felt assured, that there had been no mistake in my analysis.

Found again on repeating the analysis.

From these experiments it follows, that 100 parts of this calculus contained

Uric acid	75·33
Phosphate of lime	17·35
Animal matter	6·32
Silex	1
	100.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Natural History Society.

AT the last meeting of the Wernerian Natural History Society (July 16), the President laid before the Society three communications from Col. George Montague, F. L. S., of Knowle House, Devon. Two of these communications were read at this meeting. The first part of the first communication contained an interesting view of the natural habits and more striking external appearances of the gannet or soland goose, *pelicanus bassanus*. The second part contained an account of the internal structure of this bird, particularly of the distribution of its air-cells, which the ingenious author showed to be admirably adapted to its mode

Gannet.

mode of life, and continued residence on the water, even in the most turbulent sea, and during the most rigorous seasons. The second communication was the description and drawing of a new genus of *insect*, which inhabits the cellular membrane of the gannet, and to which Col. Montague gives the name of *cellularia bassani*.—At the same meeting, Mr. P. Neill laid before the Society a list of such fishes belonging to the four Linnean orders, apodes, jugulares, thoracici, and abdominales, as he had ascertained to be natives of the waters in the neighbourhood of Edinburgh, accompanied with valuable remarks, and illustrated by specimens of some of the rarer species. Of the *apodes* he enumerated 4 species belonging to 3 genera: 2 to muræna, 1 anarhichas, and 1 ammodytes. Of the *jugulares* he mentioned 13 species, belonging to 3 genera: 1 callionymus (the gemmeous dragonet, for, from examining many specimens, the author had concluded, that the *sordid dragonet* of Mr. Pennant and Dr. Shaw is not a distinct species, but merely the female of the gemmeous dragonet), 9 of the genus gadus, and 2 blennius. Of the *thoracici* he stated 22 species, belonging to 9 genera: 1 gobius, 2 cottus, 2 zeus, the doree and the opah (a specimen of this last most resplendent fish having been taken off Cramond in the Firth of Forth some years ago, and being still preserved in the museum of P. Walker, Esq.), 7 pleuronectes, 1 sparus, the toothed gilt head (a rare fish, of which only two specimens have occurred in the Frith of Forth), 2 perca, 3 gasterosteus, with 1 trigla. Of the *abdominales* he had ascertained 14 species, belonging to 7 genera: 1 cobitis, 4 salmo, 3 esox, the pike, garpike, and the saury or *gandanook* (which last, though rare in England, is not, he stated, uncommon at Edinburgh, but arrives in the Frith almost every autumn in large shoals), 3 clupea. Of the genus cyprinus, of which no fewer than ten species inhabit the rivers and ponds of England (including the carp, tench, gudgeon, dace, roach, bream, &c.), only one insignificant species, the author remarked, is found near Edinburgh, viz. the common minow. Of the genus scomber, the mackarel is got in the entrance of the Frith of Forth. Mr. Neill reserved the notice of the *amphibia nantes* of Linnæus, including the ray tribe, to a future meeting.

New insect.

Fishes near Edinburgh.

Sordid dragonet, the female of the gemmeous.

METEOROLOGICAL JOURNAL

For JULY, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE. Day of	THERMOMETER. 11 A. M.	11 P. M.	Highest.	Lowest.	BAROMETER.	WEATHER. Night.	Day.
29	63	64	71	56	30·14	Fair	Fair
30	63	60	69	55	30·23	Ditto	Ditto
JULY.							
1	63	60	70	56	30·16	Ditto	Ditto
2	62	61	69	56	30·06	Ditto	Ditto
3	63	60	69	51	30·02	Ditto	Rain
4	60	59	67	52	30·01	Ditto	Fair
5	61	62	67	56	29·97	Ditto	Ditto
6	61	64	68	60	30·10	Ditto	Ditto
7	68	65	72	59	30·14	Ditto	Ditto
8	68	65	72	62	30·05	Ditto	Ditto
9	64	66	71	62	30·04	Ditto	Ditto
10	65	65	72	60	30·07	Ditto	Ditto
11	68	72	76	66	30·17	Ditto	Ditto
12	74	78	83	70	30·12	Ditto	Ditto
13	80	81	87	74	30·01	Ditto	Ditto
14	82	82	87	70	30·02	Ditto	Ditto
15	76	68	81	73	30	Ditto*	Ditto
16	75	73	83	65	29·96	Ditto	Ditto
17	74	74	81	65	39·03	Ditto	Ditto
18	74	75	80	66	30·05	Ditto	Ditto
19	76	73	82	64	29·94	Ditto	Ditto
20	71	65	75	61	29·85	Ditto	Rain
21	69	65	75	60	29·75	Ditto	Fair
22	71	68	74	66	29·76	Ditto	Ditto
23	70	68	77	64	29·84	Rain	Ditto
24	73	65	81	61	29·83	Cloudy †	Rain ‡
25	65	61	68	60	29·78	Ditto	Ditto

* Lightning in the W. † Lightning in the S.E. ‡ Thunder.

☞ I have lately seen in the Papers several accounts of the great height of the thermometer in various places; and as there appears much difference in the temperatures, I conceive there must have been more or less reflected heat in the different situations. The thermometers, from which I register, hang a few feet from the ground, against a wall that has nearly an eastern aspect, and is completely sheltered from the sun both at its back and front the whole day, in such a manner, that it cannot be affected by its heat, either direct or reflected. I conclude therefore, that the highest temperature here stated is a near approximation to truth.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XX.

ARTICLE I.

The Bakerian Lecture on some new Phenomena of Chemical Changes produced by Electricity; particularly the Decomposition of the fixed Alkalis, and the Exhibition of the new Substances which constitute their Bases; and on the general Nature of Alkaline Bodies. By HUMPHRY DAVY, *Esq. Sec. R.S. M.R.I.A.*

(Concluded from Page 314.)

VII. *Some general Observations on the Relations of the Bases of Potash and Soda to other Bodies.*

SHOULD the bases of potash and soda be called metals? The greater number of philosophical persons, to whom this question has been put, have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination.

Are these bases to be called metals?

Their low specific gravity does not appear a sufficient reason for making them a new class; for among the metals themselves there are remarkable differences in this respect, platina being nearly four times as heavy as tellurium *; and in

Their lightness not a sufficient objection.

* Tellurium is not much more than six times as heavy as the bases of soda. There is great reason to believe, that bodies of a similar

in the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

Nomenclature.

On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination, which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language.

Potasium and sodium.

Potasium and sodium are the names, by which I have ventured to call the two new substances: and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an errour; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country upon the methods of derivation, and the one I have adopted has been the one most generally approved. It is perhaps more significant than elegant. But it was not possible to found names upon specific properties not common to both; and though a name for the bases of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalis.

The terms should be unconnected with theory.

The more caution is necessary in avoiding any theoretical expression in the terms, because the new electro-chemical phenomena, that are daily becoming disclosed, seem distinctly to show, that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision, than a conviction of its permanency and truth.

The discovery of the agencies of the gasses destroyed the hypothesis of Stahl. The knowledge of the powers and effects of the ethereal substances may at a future time possibly

similar chemical nature to the bases of potash and soda will be found of intermediate specific gravities between them and the lightest of the common metals. Of this subject I shall treat again in the text in some of the following pages.

act

act a similar part with regard to the more refined and ingenious hypothesis of Lavoisier; but in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of chemistry.

Whatever future changes may take place in theory, there seems however every reason to believe, that the metallic bases of the alkalis, and the common metals, will stand in the same arrangement of substances; and as yet we have no good reasons for assuming the compound nature of this class of bodies *.

Metals not likely to be separated, and no reason yet to suppose them compounds.

The experiments in which it is said, that alkalis, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner †; for distilled water, as I have endea-

Air and water not free from solid matters.

* A phlogistic chemical theory might certainly be defended, on the idea, that the metals are compounds of certain unknown bases with the same matter as that existing in hidrogen; and the metallic oxides, alkalis, and acids, compounds of the same bases with water;—but in this theory more unknown principles would be assumed than in the generally received theory. It would be less elegant and less distinct. In my first experiments on the distillation of the bases of potash, finding hidrogen generally produced, I was led to compare the phlogistic hypothesis with the new facts, and I found it fully adequate to the explanation. More delicate researches however afterward proved, that in the cases when inflammable gasses appeared, water, or some body in which hidrogen is admitted to exist, was present.

Phlogistic theory.

† The explanation of Van Helmont of his fact of the production of earth in the growth of the willow was completely overturned by the researches of Woodward. Phil. Trans. Vol. XXI. page 193.

Van Helmont's experiment.

The conclusions which M. Braconnot has very lately drawn from his ingenious experiments, Annales de Chemie, Fevrier 1807, page 187, [see our Journal, vol. XVIII, p. 15.] are rendered of little avail in consequence of the circumstances stated in the text. In the only case of vegetation in which the free atmosphere was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances, which might afford carbon, or various inflammable matters. Carbonaceous matter exists in several stones, which afford a whitish or grayish powder; and when in a stone the quantity of carbonate of lime is very small in proportion to the other earthy ingredients, it is scarcely acted on by acids.

Braconnot's experiments.

 voured

voured to show*, may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds.

All the products of living beings may be elicited from known combinations.

In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of the alkalis, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic †, and granitic rocks, there is a constant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants, that have been examined, certain neutrosaline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements.

Organization rather combines than decomposes.

VIII. *On the Nature of Ammonia and alkaline Bodies in general; with Observations on some Prospects of Discovery offered by the preceding Facts.*

Composition of ammonia supposed to be ascertained.

Ammonia is a substance, the chemical composition of which has always been considered of late years as most perfectly ascertained, and the apparent conversion of it into hidrogen and nitrogen, in the experiments of Scheele, Priestley, and the more refined and accurate experiments of Berthollet, had left no doubt of its nature in the minds of the most enlightened chemists.

* Bakerian Lecture, 1806, page 8.

† In the year 1804, for a particular purpose of geological inquiry, I made an analysis of the porcelain clay of St. Stevens, in Cornwall, which results from the decomposition of the feldspar of fine-grained granite. I could not detect in it the smallest quantity of alkali. In making some experiments on specimens of the undecompounded rock taken from beneath the surface, there were evident indications of the presence of a fixed alkali, which seemed to be potash. So that it is very probable, that the decomposition depends on the operation of water and the carbonic acid of the atmosphere on the alkali forming a constituent part of the chrystalline matter of the feldspar, which may disintegrate from being deprived of it.

All

All new facts must be accompanied however by a train of analogies, and often by suspicions with regard to the accuracy of former conclusions. As the two fixed alkalis contain a small quantity of oxigen united to peculiar bases, may not the volatile alkali likewise contain it? was a query which soon occurred to me in the course of inquiry; and in perusing the accounts of the various experiments made on the subject, some of which I had carefully repeated, I saw no reason to consider the circumstance as impossible. For supposing hidrogen and nitrogen to exist in combination with oxigen in low proportion, this last principle might easily disappear in the analytical experiments of decomposition by heat and electricity, in water deposited upon the vessels employed or dissolved in the gasses produced.

But conjectured to contain oxigen.

Of the existence of oxigen in volatile alkali I soon satisfied myself. When charcoal carefully burnt and freed from moisture was ignited by the Voltaic battery of the power of 250 of 6 and 4 inches square, in a small quantity of very pure ammoniacal gas*: a great expansion of the aeriform matter took place, and a white substance formed, which collected on the sides of the glass tube employed in the process; and this matter, exposed to the action of diluted muriatic acid, effervesced, so that it was probably carbonate of ammonia.

This proved.

A process of another kind offered still more decisive results. In this the two mercurial gazometers of the invention of Mr. Pepys, described in No XIV of the Phil. Trans. for 1807 †, were used with the same apparatus, as that

A more decisive proof.

* The apparatus in which this experiment was made is described in page 214 Journal of the Royal Institution. The gas was confined by mercury, which had been previously boiled to expel any moisture that might adhere to it. The ammonia had been exposed to the action of dry pure potash, and a portion of it equal in volume to 10980 grains of mercury, when acted on by distilled water, left a residuum equal to 9 grains of mercury only. So that the gas, there is every reason to believe, contained no foreign aeriform matter; for even the minute residuum may be accounted for by supposing it derived from air dissolved in the water.

† See Journal, vol. XIX, p. 217.

employed

A more decisive proof.

employed by Mrs. Allen and Pepys for the combustion of the diamond, and these gentlemen kindly assisted in the experiment.

Very pure ammoniacal gas was passed over iron wire ignited in a platina tube, and two curved glass tubes were so arranged, as to be inserted into a freezing mixture; and through one of these tubes the gas entered into the platina tube, and through the other it passed from the platina tube into the airholder arranged for its reception.

The temperature of the atmosphere was 55°; and it was observed, that no sensible quantity of water was deposited in the cooled glass tube transmitting the unaltered ammonia, but in that receiving it after its exposure to heat moisture was very distinct, and the gas appeared in the airholder densely clouded.

This circumstance seems distinctly to prove the formation of water in this operation for the decomposition of ammonia; unless indeed it be asserted, that the hidrogen and nitrogen gasses evolved hold less water in solution or suspension than the ammonia decomposed, an idea strongly opposed by the conclusions of Mr. Dalton * and the experiments of Messrs. Desormes and Clement †.

After the gas had been passed several times through the ignited tube from one gazometer to the other, the results were examined. The iron wire became converted superficially into oxide, and had gained in weight $\frac{44}{100}$ parts of a grain, about $\frac{4}{10}$ of a grain of water were collected from the cooled glass tubes by means of filtrating paper, and 33·8 cubic inches of gas were expanded into 55·3 cubic inches, and by detonation with oxigen it was found, that the hidrogen gas in these was to the nitrogen as 3·2 to 1 in volume.

It will be useless to enter into the more minute details of this experiment, as no perfectly accurate data for proportions can be gained from them; for the whole of the ammonia was not decomposed, and as the gas had been prepared by being sent from a heated mixture of sal ammoniac and quicklime into the airholder, it was possible, that some solution of

* Manchester Memoirs, Vol. V, Part II, page 535, 1785.

† Annales de Chemie, Vol. XLII, p. 125.

ammonia

ammonia might have been deposited, which, by giving out new gas during the operation, would increase the absolute quantity of the material acted upon.

Berthollet's decomposition of ammonia by electricity. Products exceeded.

In examining the results of Mr. Berthollet's* elaborate experiments on the decomposition of ammonia by electricity, I was surprised to find, that the weight of the hidrogen and nitrogen produced rather exceeded than fell short of that of the ammonia considered as decomposed, which was evidently contradictory to the idea of its containing oxigen. This circumstance, as well as the want of coincidence between the results and those of Priestley and Van Marum on the same subject, induced me to repeat the process of electrization of ammonia, and I soon found, that the quantities of the products in their relations to the apparent quantity of gas destroyed were influenced by many different causes.

Quantities of the products influenced by various causes.

Ammonia procured over dry mercury from a mixture of dry lime and muriate of ammonia, I found, deposited moisture upon the sides of the vessel, in which it was collected, and in passing the gas into the tube for electrization, it was not easy to avoid introducing some of this moisture, which must have been a saturated solution of ammonia, at the same time.

In my first trials, made upon gas passed immediately from the vessel in which it had been collected into the apparatus, I found the expansion of 1 of ammonia vary in different instances from 2·8 to 2·2 measures, but the proportions of the nitrogen and hidrogen appeared uniform, as determined by detonation of the mixed gas with oxigen, and nearly as 1 to 3 in volume.

To exclude free moisture entirely, I carefully prepared ammonia in a mercurial airholder, and after it had been some hours at rest, passed a quantity of it into the tube for decomposition, which had been filled with dry mercury. In this case 50 parts became 103 parts by electrization, and there was still reason to suspect sources of errour.

I had used iron wires not perfectly free from rust for taking the spark, and a black film from the mercury appeared on the sides of the tube. It was probable, that some ammonia had been absorbed by the metallic oxides both upon the

* *Mémoires de l'Académie*, 1785, page 324.

iron

iron and the mercury, which might again have been given out in the progress of the operation.

I now used recently distilled mercury, which did not leave the slightest film on the glass tube, and wires of platina. The ammonia had been exposed to dry caustic potash, and proved to be equally pure with that mentioned in page 326. 60 measures of it, each equal to a grain of water, were electrized till no farther expansion could be produced, the gas filled a space equal to that occupied by 108 grains of water. The thermometer in this experiment was at 56°, and the barometer at 30·1 inches. The wire of platina transmitting the spark was slightly tarnished *. The 108 measures of gas, carefully analyzed, were found to consist of 80 measures in volume of hidrogen, and 28 measures of nitrogen.

Specific gravity of ammonia.

The results of an experiment that I made in 1799 † give the weight of 100 cubic inches of ammonia as 18·18 grains at the mean temperature and pressure. I had reasons however for suspecting, that this estimation might be somewhat too low, and on mentioning the circumstance to Messrs. Allen and Pepys, they kindly undertook the examination of the subject, and Mr. Allen soon furnished me with the following data. "In the first experiment 21 cubic inches of ammonia weighed 4·05 grains; in a second experiment the same quantity weighed 4·06 grains, barometer 30·65, thermometer 54° Fahrenheit."

Now if the correctness for temperature and pressure be made for these estimations, and a mean taken, 100 cubic inches of ammonia will weigh 18·67 grains, barometer being at 30, and thermometer at 60° Fahrenheit: and if the quantity used in the experiment of decomposition be calculated upon as cubic inches, 60 will weigh 11·2 grains. But the hidrogen gas evolved equal to 80 will weigh 1·93 ‡ grains, and the nitrogen equal to 28 §, 8·3. And 1·9 +
8·3

* This most probably was owing to oxidation. When platina is made positive in the Voltaic circuit in contact with solution of ammonia, it is rapidly corroded. This is an analogous instance.

† Researches Chem. and Phil. p. 62.

‡ Lavoisier's Elements, p. 569. A cubical inch of hidrogen is considered as weighing ·0239.

§ Researches Chem. and Phil. page 9. From my experiments 100

8·3=10·2; and 11·2—10·2=1; all the estimations being made according to the standard temperature and pressure.

So that in this experiment on the decomposition of ammonia, the weight of the gasses evolved is less by nearly $\frac{1}{11}$ than that of the ammonia employed; and this loss can only be ascribed to the existence of oxigen in the alkali; part of which probably combined with the platina wires employed for electrization, and part with hidrogen.

Products only $\frac{10}{11}$, therefore $\frac{1}{11}$ oxigen.

After these ideas the oxigen in ammonia cannot well be estimated at less than 7 or 8 parts in the hundred; and it possibly exists in a larger proportion, as the gasses evolved may contain more water than the gas decomposed, which of course would increase their volume and their absolute weight*.

Ammonia probably contains more than ·08.

In supposing ammonia a triple compound of nitrogen, hidrogen, and oxigen, it is no less easy to give a rational account of the phænomena of its production and decomposition, than in adopting the generally received hypothesis of its composition.

Supposing it a triple compound, the phenomena easily accounted for.

Oxigen, hidrogen, and nitrogen are always present in cases in which volatile alkali is formed; and it usually appears during the decomposition of bodies in which oxigen is loosely attached, as in that of the compounds of oxigen and nitrogen dissolved in water.

At common temperatures under such favourable circumstances, the three elements may be conceived capable of combining, and of remaining in union: but at the heat of ignition the affinity of hidrogen for oxigen prevails over the complex attraction, water is formed, and hidrogen and nitrogen are evolved; and according to these conclusions, ammonia will bear the same relations to the fixed alkalis, as the vegetable acids with compound bases do to the mineral ones with simple bases.

Ammonia analogous to the acids with compound bases.

100 cubical inches of nitrogen weigh, at the standard temperature and pressure, 29·6 grains.

* In the present state of our knowledge, perfectly correct data for proportions cannot probably be gained in any experiments on the decomposition of ammonia, as it seems impossible to ascertain the absolute quantity of water in this gas; for electrization, according to Dr. Henry's ingenious researches, offers the only means known of ascertaining the quantity of water in gasses.

Quantity of water in gasses to be known only by electrization.

5 Oxigen

Oxigen the principle of alkalinity.

Oxigen then may be considered as existing in, and as forming an element in all the true alkalis; and the principle of acidity of the French nomenclature might now likewise be called the principle of alkalescence.

The alkaline earths probably oxidized metals.

From analogy alone it is reasonable to expect, that the alkaline earths are compounds of a similar nature to the fixed alkalis, peculiar highly combustible metallic bases united to oxigen. I have tried some experiments upon barytes and strontities; and they go far towards proving, that this must be the case. When barytes and strontites, moistened with water, were acted upon by the power of the battery of 250 of 4 and 6, there was a vivid action and a brilliant light at both points of communication, and an inflammation at the negative point.

Barytes and strontia appear to be so.

In these cases the water might possibly have interfered. Other experiments gave however more distinct results.

Barytes and strontites, even when heated to intense whiteness in the electrical circuit by a flame supported by oxigen gas, are nonconductors; but by means of combination with a very small quantity of boracic acid, they become conductors; and in this case inflammable matter, which burns with a deep red light in each instance, is produced from them at the negative surface. The high temperature has prevented the success of attempts to collect this substance; but there is much reason to believe, that it is the bases of the alkaline earth employed.

Inflammable matter produced from them.

Barytes and strontites have the strongest relations to the fixed alkalis of any of the earthy bodies*; but there is a chain of resemblances, through lime, magnesia, glucina, alumina, and silex. And by the agencies of batteries sufficiently strong, and by the application of proper circum-

Probably other earths may be analyzed by electricity.

Earths long ago considered analogous to metallic oxides.

* The similiarity between the properties of earths and metallic oxides was noticed in the early periods of chemistry. The poisonous nature of barytes, and the great specific gravity of this substance as well as of strontites, led Lavoisier to the conjecture, that they were of a metallic nature. That metals existed in the fixed alkalis seems however never to have been suspected. From their analogy to ammonia, nitrogen and hidrogen have bèen supposed to be amongst their elements. It is singular, with regard to this class of bodies, that those most unlike metallic oxidés are the first which have been demonstrated to be such.

stances

stances, there is no small reason to hope, that even these refractory bodies will yield their elements to the methods of analysis by electrical attraction and repulsion.

In the electrical circuit we have a regular series of powers of decomposition, from an intensity of action, so feeble as scarcely to destroy the weakest affinity existing between the parts of a saline neutral compound, to one sufficiently energetic to separate elements in the strongest degree of union in bodies undecomposable under other circumstances. Powers of electricity form a regular series.

When the powers are feeble, acids and alkalis, and acids and metallic oxides, merely separate from each other; when they are increased to a certain degree, the common metallic oxides and the compound acids are decomposed; and by means still more exalted, the alkalis yield their elements. And as far as our knowledge of the composition of bodies extends, all substances attracted by positive electricity are oxigen, or such as contain oxigen in excess; and all that are attracted by negative electricity are pure combustibles, or such as consist chiefly of combustible matter. Their action. Oxigen attracted by positive electricity, combustible matter by negative.

The idea of muriatic acid, fluoric acid, and boracic acid containing oxigen, is highly strengthened by these facts. And the general principle confirms the conjecture just stated concerning the nature of the earths.

In the electrization of boracic acid moistened with water, I find, that a dark coloured combustible matter is evolved at the negative surface; but the researches upon the alkalis have prevented me from pursuing this fact, which seems however to indicate a decomposition. Boracic acid.

Muriatic acid and fluoric acid in their gaseous states are nonconductors: and as there is every reason to believe, that their bases have a stronger attraction for oxigen than water, there can be little hope of decomposing them in their aqueous solutions, even by the highest powers. In the electrization of some of their combinations there is however a probability of success. Muriatic and fluoric acid.

An immense variety of objects of research is presented in the powers and affinities of the new metals produced from the alkalis. New metals afford a large field of research.

In

An instrument of decomposition.

In themselves they will undoubtedly prove powerful agents for analysis; and having an affinity for oxigen stronger than any other known substances, they may possibly supersede the application of electricity to some of the undecompounded bodies.

Base of potash decomposes carbonic acid.

The bases of potash I find oxidates in carbonic acid and decomposes it, and produces charcoal when heated in contact with carbonate of lime. It likewise oxidates in muriatic acid; but I have had no opportunity of making the experiment with sufficient precision to ascertain the results.

Geology.

In sciences kindred to chemistry, the knowledge of the nature of the alkalis, and the analogies arising in consequence, will open many new views; they may lead to the solution of many problems in geology, and show, that agents may have operated in the formation of rocks and earths, which have not hitherto been suspected to exist.

It would be easy to pursue the speculative part of this inquiry to a great extent, but I shall refrain from so occupying the time of the Society, as the tenour of my object in this lecture has not been to state hypotheses, but to bring forward a new series of facts.

II.

On the Composition of the Compound Sulphuret from Huel Boys, and an Account of its Crystals. By James Smithson, *Esq. F. R. S.**.

Compound sulphuret from Huel Boys.

It is but very lately, that I have seen the Philosophical Transactions for 1804, and become acquainted with the two papers on the compound sulphuret of lead, antimony, and copper contained in the first part of it †; which circumstance has prevented my offering sooner a few observations on Mr. Hatchett's experiments, which I deem essential towards this substance being rightly considered, and indeed the principles of which extend to other chemical compounds; and also giving an account of this compound sulphuret,

* Philos. Trans. for 1807, Part 1, p. 55.

† See Journal, vol. IX, p. 14.

Compound Sulphuret

Fig. 1

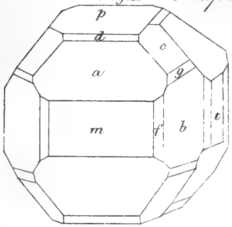
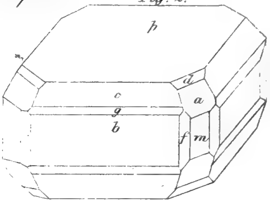
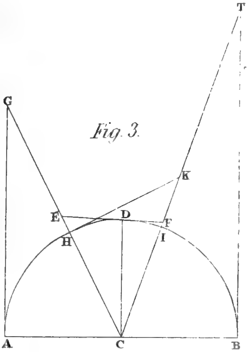


Fig. 2



New Projection of Tangents

Fig. 3.



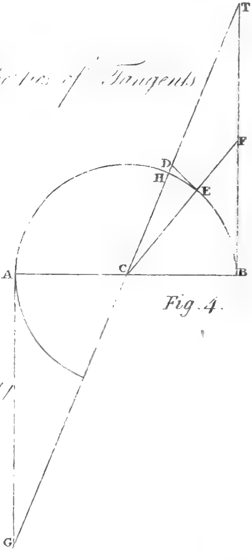


Fig. 4.

Radiation & Reflection of Cold

Fig. 5.

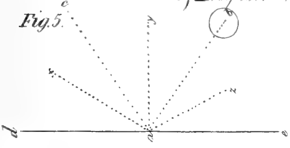
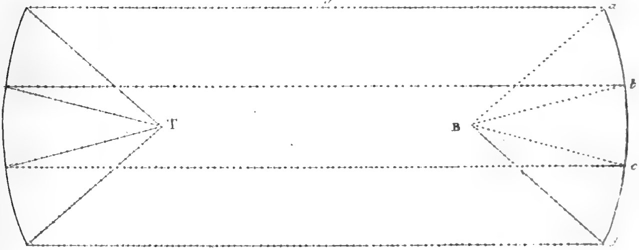


Fig. 6.



phuret, as that which had been laid before the Society is very materially inaccurate and imperfect.

We have no real knowledge of the nature of a compound substance, till we are acquainted with its proximate elements, or those matters by the direct or immediate union of which it is produced; for these only are its true elements. Thus, though we know that vegetable acids consist of oxigen, hidrogen, and carbon, we are not really acquainted with their composition, because these are not their proximate, that is, are not their elements, but are the elements of their elements, or the elements of these. It is evident what would be our acquaintance with sulphate of iron, for example, did we only know that a crystal of it consisted of iron, sulphur, oxigen, and hidrogen; or of carbonate of lime, if only that it was a compound of lime, carbon or diamond, and oxigen. In fact, totally dissimilar substances may have the same ultimate elements, and even probably in precisely the same proportions; nitrate of ammonia, and hydrate of ammonia, or crystals of caustic volatile alkali*, both ultimately consist of oxigen, hydrogen, and azote.

To know the nature of a compound we must find its proximate elements.

It is not probable, that the present ore is a direct quadruple combination of the three metals and sulphur, and that these, in their simple states, are its immediate component parts; it is much more credible, that it is a combination of the three sulphurets of these metals.

The ore probably composed of three sulphurets.

On this presumption I have made experiments to determine the respective proportions of these sulphurets in it.

I have found 10 grains of galena, or sulphuret of lead, to produce 12·5 grains of sulphate of lead. Hence the 60·1 grains of sulphate of lead, which Mr. Hatchett obtained, correspond to 48·08 grains of sulphuret of lead.

10 grs sulphuret of lead produce 12·5 sulphate.

I have found 10 grains of sulphuret of antimony to afford 11 grains of precipitate from muriatic acid by water. Hence 31·5 grains of this precipitate are equal to 28·64 grains of sulphuret of antimony.

10 grs sulphuret of antimony 11 grs sulphate.

The want of sulphuret of copper has prevented my determining the relation between it and black oxide of copper,

* Fourcroy, *Syst. des Con. Chem. t.* I. p. lxxxviii. Transl. 1, 100.

but

but this omission is, it is evident, immaterial, as the quantity of this sulphuret in the ore must be the complement of the sum of the two others.

But as the iron is a foreign adventitious substance in this ore, it follows that the foregoing quantities are the products of only 96·65 grains of it. 100 parts of the ore are thereore composed of

Component parts of the ore:

Sulphuret of lead -	49·7
Sulphuret of antimony	29·6
Sulphuret of copper -	20·7
	100·0

—or probably in more simple proportions.

It is impossible not to be struck with the trifling alteration which these quantities require, to reduce them to very simple proportions, or to think it a very great violation of probability to suppose that experiments, affected with no errours, would have given them thus:

Sulphuret of lead -	50.
Sulphuret of antimony -	30.
Sulphuret of copper. -	20.

But perhaps no substance more than two proximate elements.

However, I doubt the existence of triple, quadruple, &c. compounds; I believe, that *all combination is binary;* that no substance whatever has more than two proximate or true elements; and hence I should be inclined to consider the present compound as a combination of galena and fahlertz; and if so, it will be accurately represented, as far as chemical analysis has yet been able to go by the following figure:

True nature of the compound.

$$\begin{array}{l}\text{Compound}\\ \text{sulphuret}\\ \text{of lead,}\\ \text{antimony,}\\ \text{and copper}\end{array} = \left\{\begin{array}{l} \frac{1}{2}\text{ galena} = \left\{\begin{array}{l}\frac{1}{6}\text{ sulphur}\\ \frac{5}{6}\text{ lead}\end{array}\right.\\ \\ \frac{1}{2}\text{ fahlertz} = \left\{\begin{array}{l}\frac{3}{5}\text{ sulphuret of antimony} = \left\{\begin{array}{l}\frac{1}{6}\text{ sulphur.}\\ \frac{5}{6}\text{ antimony.}\end{array}\right.\\ \frac{2}{5}\text{ sulphuret of copper} = \left\{\begin{array}{l}\frac{1}{3}\text{ sulphur.}\\ \frac{2}{3}\text{ copper.}\end{array}\right.\end{array}\right.\end{array}\right.$$

Ultimate elements,

Its ultimate elements are therefore,

Sulphur -	20	. . . $=\frac{12}{60}$
Lead -	$41\frac{2}{3}$.	. . $=\frac{25}{60}$
Antimony -	25	. . . $=\frac{15}{36}$
Copper -	$13\frac{1}{3}$.	. . $=\frac{8}{60}$

and

and it is not a little remarkable, that here, as was the case with the calamine*, they are sexagesimal fractions of it. In sexagesimal fractions.

When in a former paper I offered a system on the proportions of the elements of compounds, I supported it by the results of my own experiments, which might be supposed influenced, even unconsciously to myself, by a favourite hypothesis, and I made the application of it principally to a substance, the nature of which was not very clear. But the present case is not liable to these objections: here no fondness to the theory can be suspected of having led astray, nor did even the experiments, as they came from their author's hands, bear an appearance in the least favourable to it, and yet when properly considered, they are found to accord no less remarkably with its principles.

Elements of compounds must unite in precise quantities.

It is evident, that there must be a precise quantity, in which the elements of compounds are united together in them, otherwise a matter, which was not a simple one, would be liable, in its several masses, to vary from itself, according as one or other of its ingredients chanced to predominate; but chemical experiments are unavoidably attended with too many sources of fallacy for this precise quantity to be discovered by them; it is therefore to theory, that we must owe the knowledge of it. For this purpose an hypothesis must be made, and its justness tried by a strict comparison with facts. If they are found at variance, the assumed hypothesis must be relinquished with candour as erroneous: but should it on the contrary prove, on a multitude of trials, invariably to accord with the results of observation, as nearly as our means of determination authorise us to expect, we are warranted in believing, that the principle of nature is obtained; as we then have all the proofs of its being so, which men can have of the justness of their theories; a constant and perfect agreement with the phenomena, as far as can be discovered.

Hypothesis necessary but must be tested by facts.

Do the simple ratios required by the theory always obtain?

The great criterion in the present case is, whether on the conversion of a substance into its several compounds, and of these into one another, the simple ratios always obtain, which the principles of the theory require. Amongst the

* Phil. Trans. 1803, p. 12. or Journal, vol. VI, p. 83.

multitude

multitude of instances which I could adduce, in support of such being the fact, I will for the sake of brevity confine myself to a few, in the substances which have come under consideration above, as they will likewise give the grounds, on which some of the proportions in the table have been assigned, and every chemist, by a careful repetition of the experiments, may easily determine for himself to what attention the present theory is entitled.

Instances.

Lead - -	$= \frac{3}{2}$ of sulphate of lead
	$= \frac{6}{5}$ of sulphuret of lead
Sulphuret of lead	$= \frac{5}{6}$ of lead
	$= \frac{5}{4}$ of sulphate of lead
Sulphate of lead	$= \frac{2}{3}$ of lead
	$= \frac{4}{5}$ of sulphuret of lead
Antimony -	$= \frac{4}{3}$ of powder of algaroth
	$= \frac{6}{5}$ of sulphuret of antimony
Sulphuret of antimony -	$= \frac{10}{9}$ of powder of algaroth.

In the experiments by which these relations were ascertained, the portion of powder of algaroth and sulphate of lead dissolved in the precipitating and washing waters was scrupulously collected.

Perhaps the quantity of an element expresses its force of attraction.

The importance of a knowledge of the true quantity in which matters combine is too evident, to require to be dwelt upon; but this importance will be greatly augmented, if it should prove, that this quantity is, as has been suggested, expressive of the forces with which they attract each other. It is perhaps in the form of matters, that we shall find the cause of the proportions in which they unite, and a proof, *a priori*, of the system here maintained.

Gray copper ore.

I have examined some of the gray ores of copper in tetraedral crystals; but the notes of my experiments are in England. I can however, say, that they do contain antimony, and that they do not contain iron in any material quantity. With respect to the proportions of the constituent parts, I cannot now speak with any certainty; but, I think, that at least some species of fahlertz contain a smaller portion of sulphuret of antimony, than the fahlertz does which exists as an element in the foregoing compound one.

Of

Of the Form of this Substance.

Of the seventeen figures which have been given, as of the crystals of this compound sulphuret, in Part II of the volume of the Transactions for 1804, great part are acknowledged to have no existence, nor are indeed any of them consistent with nature. Form of the compound sulphuret.

This substance seems to have yet offered but one form, which is represented in Plate 9 under its two principal appearances; that is, having the primitive faces the predominant ones of the prism; and having the secondary ones such, and which will be fully sufficient to make it known. In the first infancy of the study of crystals, it might be necessary to attend to every, the most trifling, variation of them, to trace each of their changes step by step, to spell as it were, the subject; but in the state to which the science has now attained, to continue to do so would be not only superfluous, but most truly puerile.

I have a very small, but very regular, crystal of the form of Fig. 1.

By mensuration the faces *a* and *m* appear to form together an angle of about 135°, and the faces *c* and *b* an angle of about 125°.

It is said in the account above quoted, that the primitive form of this matter is a rectangular tetraedral prism, but no proofs of this have been offered; nor have the dimensions of this prism been given, a circumstance of the first moment to the determination of true or primitive form, nor have any quantities been assigned to the decrements supposed. I will, therefore, supply these very important omissions. Dimensions of the prism necessary to determine the primitive form.

That the atom of this substance is a rectangular tetraedral prism, is inferable, not from the striæ on the crystals, for striæ are by no means invariably indicative of a decrement in the direction of them; but from the angles which the faces *a* and *c* make with the faces *m* and *b*; and these angles also prove, that the height of this prism is equal to the side of its base, that is, that it is a cube. It is a cube.

Hence the face *a* is produced by a decrease of one row of atoms along the edge of the cube, and the angle it forms with the face *m* is really of 135°. Its decrements.

The face c is produced by a decrease of two rows of atoms at the corners of the cube, and the angle it forms with the face b is $= 125° 15' 52''$.

The face b being produced like the face a, forms the same angle with the face m.

No crystal I possess has enabled me to measure the inclinations of the faces g, d, or f; should the face g, as is presumable, result from a decrease of one row of atoms at the corners of the cube, it will form with the face b an angle of $144° 44' 8''$; and if the faces d and f are, as is also probable, produced by a decrease of two rows of atoms along the edges of the cube, the first will form an angle of $116° 33' 54''$, and the latter one of $153° 26' 6''$, with the face m.

This differs from the former account of the crystals.

The angles assigned here differ considerably from those given in the former account of these crystals; but the angles there given have not only appeared to me to be contradicted by observation, but, crystallographically considered, are inconsistent with each other, as the tetraedral prism of dimensions to produce an angle of 135° by a decrement along its edge would not afford angles of 140° and 120° by decrements at its corners.

The sum of the faces of these crystals is 50.

III.

On a new Property of the Tangents of the three Angles of a Plane Triangle. By Mr. William Garrard, *Quarter Master of Instruction at the Royal Naval Asylum at Greenwich. Communicated by the Astronomer Royal**.

Sum of three tangents of a plane triangle multiplied by square of radius equal to their continued product. Demonstrated in an acute angled triangle:

PROPOSITION I. In every acute angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of the radius is equal to the continued product of the tangents.

Demonstration.—Let AH, HI, and IB, Plate 9 Fig. 3, be the arches to represent the given angles; and AG, HK, and BT be their tangents, put r the radius, $AG=a$, and $BT=b$,

* Philos. Trans. for 1807, Part I, p. 120.

Then

Then $\frac{r^2}{a}$ and $\frac{r^2}{b}$ will be the tangents of HD and DI.

Now by Prop. VIII, Sect. I, Book I, Emerson's Trigonometry,

As radius square—product of two tangents
Is to radius square,
So is the sum of the tangents
To the tangent of their sum.

$$\therefore r^2 - \frac{r^4}{ab} : r^2 :: \frac{r^2}{a} + \frac{r^2}{b} : \frac{r^2a + r^2b}{ab - r^2} = \text{HK};$$

therefore $a + b + \frac{r^2a + r^2b}{ab - r^2} = \frac{a^2b + ab^2}{ab - r^2}$ = the sum of the three tangents,

and $\frac{a^2b + ab^2}{ab - r^2} \times r^2 = ab \times \frac{r^2a + r^2b}{ab - r^2}$ = their continued product. Q. E. D.

Proposition II. In every obtuse angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of the radius is equal to their continued product.

Demonstration.—Let AH, Fig. 4, be an obtuse arc, and HE, EB the other two. and in an obtuse angled triangle.

Then BF, ED, and AG are the three tangents.

Put BF $= t$ and DE $= u$ radius $= r$, then per trigonometry, as before, $r^2 \times \frac{t + u}{r^2 - tu} = \text{BT}$;

But $- \text{BT} = \text{AG} = - \frac{t + u}{r^2 - tu} \times r^2$.

Wherefore $t + u - \frac{t + u}{r^2 - tu} \times r^2$ = the sum of the three tangents, which being reduced

is $= - tu \times \frac{t + u}{r^2 - tu}$, and multiplied into r^2 is equal to

$tu \times - \frac{t + u}{r^2 - tu} \times r^2$ = the product. Q. E. D.

IV.

On a new Property of the Tangents of three Arches trisecting the Circumference of a Circle. By NEVIL MASKELYNE, *D. D. F. R. S. and Astronomer Royal* *.

Sum of three tangents of three arches trisecting a circle, multiplied by radius, equal to their product.

MR. William Garrard having shown me a curious property of the tangents of the three angles of a plane triangle, or in other words, of the tangents of three arches trisecting a semicircle, in a paper which I have communicated to this Society, I was led to consider, whether a similar property might not belong to the tangents of three arches trisecting the whole circumference; and, on examination, found it to be so.

Let the circumference of a circle be divided any how into three arches A, B, C; that is, let A + B + C be equal to the whole circumference. I say, the square of the radius multiplied into the sum of the tangents of the three arches A, B, C, is equal to the product of the tangents multiplied together. I shall demonstrate this by symbolical calculation, now commonly called (especially by foreign mathematicians) analytic calculation.

Preliminary remark.

It may be proper to premise, that the signification of the symbolical expressions of the tangents of an arc, whether with respect to geometry or numbers, are to be understood according to their position as lying on one side, or the other side of the radius, passing through the point of commencement of the arc of the circle; those tangents which belong to the first or third quadrant of the circle being considered as positive, and those belonging to the second and fourth quadrant, being of a contrary direction, as negative; in like manner as the sines in the first semicircle are considered as positive, and in the second semicircle as negative; and the cosines in the first and fourth quadrant are considered as positive, and in the second and third quadrants as negative; they lying, in the second case, on the contrary side of the diameter passing through the point of ninety degrees, to what they do in the former. Hence it easily follows, that the tan-

* Ibid. p. 122.

gent

gent of any arch and of its supplement to the whole circumference, or 360 degrees, are equal and contrary to one another, or the one negative of the other.

Let t, u, w, be put for the tangents of the three arches A, B, C respectively, and r for the radius, and $\odot$ for the whole circumference. Then $A+B+C=\odot$, and $C=\odot-\overline{A+B}$. Demonstration.

By trigonometry, $t, \overline{A+B}=\frac{r^2\times\overline{t+u}}{r^2-tu}$, and the tang. C = tang. $(\odot-\overline{A+B})=-\text{tang.}\,\overline{A+B}$, by what has been said above.

Therefore $t, A+t, B+t$, C or $t+u+w=t+u-\frac{r^2\times\overline{t+u}}{r^2-tu}$ $=tu\times-\frac{r^2\times\overline{t+u}}{r^2-tu}$; but t and u are the expressions for the tangents of A and B respectively, and $-\frac{r^2\times\overline{t+u}}{r^2-tu}$ is the expression for the tangent of C, or for w. Therefore, $r^2\times\overline{t+u+w}$, or the square of the radius multiplied into the sum of the three tangents of A, B, and C $=tuw$, or the product of the tangents. Q. E. D.

V.

On the apparent Radiation and Reflection of Cold by means of two concave metallic Mirrors. In a Letter from Mr. JOHN MARTIN.

To Mr. NICHOLSON,

SIR,

THERE are many phenomena, exhibited to the notice of the chemical philosopher in the course of his arduous research, that are not so well understood as perhaps the present state of science might lead him to expect. Some of these phenomena have hitherto been totally inexplicable; others have not been explained with all the clearness and perspicuity that could be wished. Among the number of the latter may be ranked the apparent radiation and reflection of cold by means of two concave metallic mirrors. Some chemical facts not sufficiently explained. Apparent radiation and reflection of cold.

This

This curious fact, notwithstanding we are so well acquainted with the laws that govern heat during its passage through and impingency upon bodies, has never, I believe, been illustrated with sufficient clearness.

The cold body supposed to receive heat from the thermometer.

The explanations that have hitherto been given rest principally for support on the supposition, that the thermometer placed in the focus of one mirror acts as a heated body, and that the heat radiating from it is transmitted to the cold body in the opposite focus.

But this cannot give out radiant heat.

The thermometer, however, is in fact not a heated body, since it is not hotter than the surrounding atmosphere, and consequently cannot radiate caloric: but it is said, the surrounding air becomes cooled, and consequently the thermometer in respect to it is a hot body, and radiates caloric accordingly. This however does not explain clearly why the thermometer should be reduced to a temperature lower than the air which surrounds it, which will be found to be the case; or at least, it leaves too much to be supplied by the imagination. I trust I shall be able to render this matter clearer.

Another mode of accounting for it.

There are only two ways, in which heat can be made to move in one direction through any given body, we will suppose a wire A $\underline{\;x\;\;y\;\;z\;}$ B; one is the application of a superior temperature to B, causing the heat to move on towards A by the conducting power of the wire, and the tendency of the caloric to establish an equilibrium; the other is, to reduce the temperature at A, and thus cause a partial vacuity of heat, which must of necessity be filled up by a fresh quantity from toward x, which will receive again a fresh supply from toward y, and that from towards z, &c., and by this means induce a current of heat from B to A, till an equilibrium is established. It is upon this principle, the filling up of partial vacuities of heat (if I may be allowed the expression), that the rational explanation of the phenomenon in question can be grounded. Fact puts this sufficiently beyond a doubt, and it now remains to show how it is effected.

How this is effected.

It will scarcely be necessary to mention in this place, that, when a particle of heat impinges upon a plane reflecting surface, it is thrown off in an angle equal to that with

with which it is thrown upon it. Now, on the contrary, if a cold body, *b*, Fig. 5, Pl. 9, be brought near a plane reflecting surface, as particles of heat are entering into that body in all directions from the surrounding air, some particles of heat must be entering into it in the direction *a b*, consequently the point *a* of the reflecting surface must become cooler, or, to use my former expression, a vacuity of heat will be there formed: now it may be demonstrated, that this vacuity or space will not be supplied by heat moving in the direction *x a*, *y a*, or *z a*, but will be supplied by heat moving in no other direction than *c a*, which heat, striking against the point *a*, will be thrown off into the body *b*; the angle *c a d* being equal to the angle *b a e*, and bodies will move in the direction in which they meet with the least resistance; for if heat were to come from any other direction but *c a*, it would not be reflected towards the body *b*, but elsewhere, and consequently, to join the current of heat *a b*, it must again change its course. Hence it follows, that, when a cold body is brought near a plane reflecting surface, in proportion as the surrounding air becomes cool, heat will enter into that body in right lines tending to its centre; the plane reflecting surface will have its temperature lowered, and particles of heat will strike upon every part of it in such directions, as to be thrown off in right lines to the cool body.

This applied to a concave surface.

The application of this fact to the explanation of the phenomenon in question will be readily perceived, substituting concave reflecting surfaces instead of plane ones: the heat enters into the cold body placed in the focus of one mirror (B. fig. 6) from the surrounding air in all directions, consequently every point of the surface of the mirror, *a*, *b*, *c*, *d*, &c., becomes cooled, and those points can only receive a fresh supply in parallel rays, in a direct course from the opposite mirror, because only such rays (striking against so many imaginary tangents *a*, *b*, *c*, *d*, of that mirror) can be thrown off towards the body B; the opposite mirror therefore becomes cool, and for the same reason the whole surface of it must be supplied by heat from the thermometer T, which consequently must become cooler than a body placed any where in its neighbourhood.

If

If you think proper, an insertion of this explanation in your valuable Journal will greatly oblige,

Sir,

Your most obedient Servant,

Old Broad Street, JOHN MARTIN.
19 *July*, 1808.

VI.

Description of a Balance Level, useful for laying out Land for Irrigation, for Roads, and other Purposes. By Mr. Richard Drew, *of Great Ormond Street* *.

SIR,

Balance level useful in draining and watering land.

HEREWITH you will receive a Balance Level, of my invention, which I have satisfactorily used on several gentlemen's estates in Devonshire, where I have been employed to drain and carry water to irrigate meadow land. I have made several for persons in that county, whose employment is to drain and irrigate land, and they have found it to answer their purpose better than the spirit or water level, it being more portable and ready to the sight.

I have lately used it on Mr. Satterley's farm, at Hastings, to carry the water of his closes over several acres of dry ground. Dr. De Salis, who has seen it, advised me to send it to the Society of Arts, &c., that they might judge of its merits.

I am, Sir,

Your obedient Servant,

RICHARD DREW.

Explanation of the Method of using the Instrument.

Method of using it.

Set it on a triangular staff, and point it at the object staff, which is held by another person at a distance; move the level on the joint, until the inner tube plays clear within

* Trans. of the Society of Arts for 1807, p. 22. The Society voted Mr. Drew ten guineas for this invention.

the

Mr. Richard Drew's Balance Level.

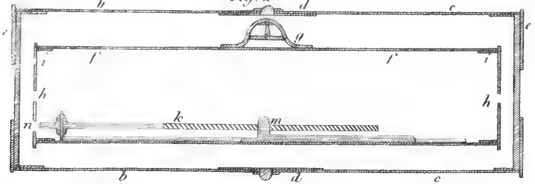


Fig. 2

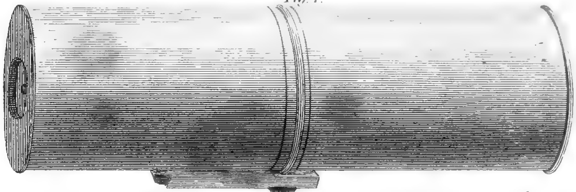


Fig. 1.

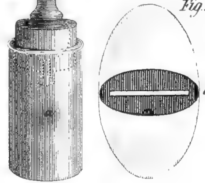


Fig. 4.

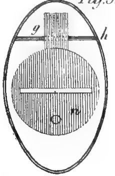


Fig. 3.

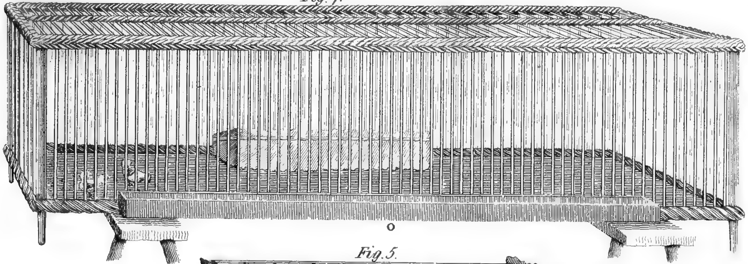


Fig. 7.

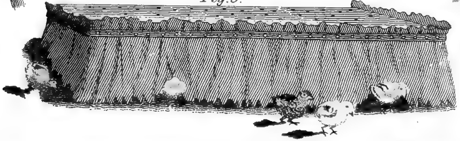


Fig. 5.

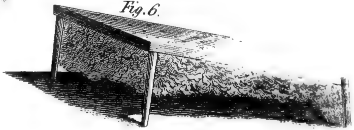


Fig. 6.

Mrs. D'Oyley's Method of rearing Poultry.

the outer tube. Look through the sights, and observe the object staff which the person holds, let him move the slide on the staff, until you see the hair cut the middle of the slide, on which there is a black line; then turn the level round, look through the sights, and see if the hair cuts the middle of the slide as before; if it does, it will be level; but if there be a difference in both ends, the person who holds the staff must set the slide to half that difference. You are then to adjust the level by turning with a key the screw, which moves the balance contained in the bottom of the inner tube.

Certificates from Mr. J. W. Gooch, Mr. Charles Layton, and Mr. Benj. Holmes, testify, that they have seen in use the level invented by Mr. Richard Drew, and that the business is done by it with accuracy and dispatch. Is used with quickness and accuracy.

Reference to the Engraving of Mr. Richard Drew's Balance Level. Plate X, *Fig.* 1, 2, 3, 4.

Fig. 1. The balance level, mounted on a ball and socket joint, with a tube, *a*, to fix on a stand. The instrument described.

Fig. 2. A section, *b b c c* two tubes of tin, which slide on a short tube, *d d*, placed in the middle, and having an iron wire soldered round it to stiffen it, and to serve as a shoulder.

e e Two eye pieces, with glass in both, one at each end, and sliding into the tubes *b* and *c*.

f f The balance level, hanging by a sort of staple *g*, on a point fixed upright on the middle of the bar *h* (shown in Fig. 3), which is fastened across the tube *d*.

i i Two eye pieces sliding into the ends of the level *f f*, and having a narrow slit horizontally across the middle, with a hair before each, shown by the dots *h h*.

k An adjusting screw, which acts by drawing the piece *m*, (which moves in a dove-tail slide), in one end of the tube.

n The key-hole through which the screw is turned.

Fig. 4. An end view of the case and level, showing the eye pieces *i* and *e*, one within the other.

VII. *Account*

VII.

Account of a new Method of rearing Poultry to Advantage. By Mrs. HANNAH D'OYLEY, *of Sion Hill, near Northallerton, Yorkshire* *.

SIR,

Cheap and easy method of rearing poultry.

I BEG leave to communicate a most desirable method of rearing poultry, which I have proved by experience; the economy and facility, with which it may be performed, would, if generally adopted, lower the price of butchers' meat, and thereby be of essential benefit to the community at large. I keep a large stock of poultry, which are regularly fed in a morning upon steamed potatoes chopped small, and at noon they have barley; they are in high condition, tractable, and lay a very great quantity of eggs. In the poultry yard is a small building, similar to a pigeon cote, for the hens to lay in, with frames covered with net to slide before each nest; the house is dry, light, and well ventilated, kept free from dirt by having the nests and walls white-washed two or three times a year, and the floor covered once a week with fresh ashes. When I wish to procure chickens, I take the opportunity of setting many hens together, confining each to her respective nest; a boy attends morning and evening to let any off that appear restless, and to see that they return to their proper places. When they hatch, the chickens are taken away, and a second lot of eggs allowed them to set again, by which means they produce as numerous a brood as before: I put the chickens into long wicker cages, placed against a hot wall at the back of the kitchen fire, and within them have artificial mothers for the chickens to run under; they are made similar to those described by Monsieur Reaumur, in "his *Art de faire éclore et d'élever en toutes Saisons des Oiseaux domestiques de toutes Espèces*," &c., in two volumes, printed at Paris, 1751. They are made of boards about ten inches broad,

Food.

Poultry house.

Breeding.

Rearing chickens.

Artificial mother.

* Trans. of the Society of Arts for 1807, p. 24. The silver medal was voted to Mrs. D'Oyley.

broad, and fifteen inches long, supported by two feet in the front, four inches in height, and by a board at the back two inches in height. The roof and back are lined with lamb's skins dressed with the wool upon them. The roof is thickly perforated with holes for the heated air to escape; they are formed without bottoms, and have a flannel curtain in front and at the ends for the chickens to run under, which they do apparently by instinct. The cages are kept perfectly dry and clean with sand or moss. The above is a proper size for fifty or sixty new hatched chickens, but as they increase in size, they of course require a larger mother. When they are a week old, and the weather fine, the boy carries them and their artificial mother to the grass-plot, nourishes and keeps them warm, by placing a long narrow tin vessel filled with hot water at the back of the mother, which will retain its heat for three hours, and is then renewed fresh from the steamer. In the evening they are driven into their cages, and resume their station at the hot wall, till they are nearly three weeks old, and able to go into a small room, appropriated to that purpose. The room is furnished with frames similar to the artificial mothers, placed round the floor, and with perches conveniently arranged for them to roost upon.

Cages.

Airing.

When I first attempted to bring up poultry in the above way, I lost immense numbers by too great heat and suffocation, owing to the roofs of the mothers not being sufficiently ventilated, and when that evil was remedied, I had another serious one to encounter; I found chickens brought up in this way did not thrive upon the food I gave them, and many of them died, till I thought of getting coarse barley-meal, and steaming it till quite soft. The boy feeds them with this and minced potatoes alternately; he is also employed rolling up pellets of dough, made of coarse wheat flour, which he throws to the chickens to excite them to eat, thereby causing them to grow surprisingly.

Numbers lost by too great heat and closeness.

Food.

I was making the above experiments in the summer for about two months, and during that time my hens produced me upwards of five hundred chickens, four hundred of which I reared fit for the table or market. I used a great many made into pies for the family, and found them cheaper than butchers'

In two months 400 reared.

Might be sold with much profit as cheap as butchers' meat.

butchers' meat. Were I situated in the neighbourhood of London, or any very populous place, I am confident I could make an immense profit, by rearing different kinds of poultry in the above method for the markets, and selling them on an average at the price of butcher's meat.

A child might bring up some thousands in a season.

A young person of twelve or fourteen years of age might bring up in a season some thousands, and by adopting a fence similar to the improved sheep-fold, almost any number might be cheaply reared, and with little trouble. Hens kept as mine are, and having the same conveniences, will readily set four times in a season, and by setting twice each time, they would produce at the lowest calculation, eighty chickens each, which would soon make them very plentiful.

One hen might produce 80 chickens a year.

If this information should be so fortunate as to merit the approbation of the Society, I shall consider myself highly honoured, and my time as having been usefully employed.

I am, Sir,

Your most obedient Servant,

HANNAH D'OYLEY.

Farther account of the mode of managing them.

The most convenient size of an artificial mother for forty or fifty young chickens is about fifteen inches long, ten deep, four high in front, and two at the back; it is placed in a long wicker cage against a warm wall, the heat at about eighty degrees of Fahrenheit's thermometer, till the chickens are a few days old, and used to the comfort of it, after which time they run under when they want rest, and acquire warmth by crowding together. I find it advisable, to have two or three chickens among them of about a week old, to teach them to peck and eat. The meat and water is given them in small troughs fixed to the outside of the cage, and a little is strewed along from the artificial mother, as a train to the main deposit. It would have given me great pleasure, to have been able to send a specimen of my superior feed and management, if the season had been rather more advanced, for I think it is not possible for turkies and chickens to weigh heavier, to be whiter, or altogether better fed than mine are.

After

After a certain age, they are allowed their liberty, living chiefly on steamed potatoes, and being situated tolerably secure from the depredations of men and foxes, are permitted to roost in trees near the house.

According to your request, I herewith send you a rough sketch of the apparatus I use, which probably will convey an idea of the business, and not be too complicated for persons employed in poultry yards, fully to understand; but to prevent trouble and prejudice in the first onset, I think it necessary to remark, that if the chickens do not readily run under the artificial mother for want of some educated ones to teach them, it will be proper to have the curtain in front made of rabbit or hare skin, with the fur side outwards, for the warmth and comfort to attract them, afterwards they run under the flannel ones, which are preferable for common use, on account of cleanliness, and not being liable to get into the mouths of the chickens. Apparatus

I have had great amusement in rearing poultry in the above way, and if my time was not occupied with my children and other family concerns, I should most assuredly farm very largely in poultry.

Reference to the Engravings of Mrs. D'Oyley's Method of breeding Poultry, Plate X, Fig. 5, 6, 7.

Fig. 5. The apparatus called the artificial mother, with a curtain of green baize in front and at each end, and holes through the top to allow the circulation of air. described.

Fig. 6. Another view of the artificial mother, but without the curtain, in order to show its sloping direction, and interior lining of woolly sheep-skin.

Fig. 7. A wicker basket four feet long, two feet broad, and fourteen inches high, with a lid to open, and a wooden sliding bottom similar to a bird cage: the artificial mother is shown, as placed within it.

O. A trough in front to hold food for the chickens.

Remark.

As the cheapness with which fowls can be reared in this way is an object of primary consideration, it is to be regretted, that Mrs. D'Oyley has not added an account of the

the quantity of food consumed by a certain number of chickens in a given time; as on this must depend the price at which they could be sold, and the profit that might be made of them. This would have been attended with another advantage, it would have been a guide with respect to the quantity of the different kinds of food, with which the chickens ought to be supplied in the several stages of their growth, to those who have not been in the habit of rearing poultry; and this must necessarily be the case with many persons in the vicinity of London in particular, to whom the adoption of Mrs. D'Oyley's plan might be very desirable. Mrs. D'Oyley does not say whether the turkeys she mentions were reared in the same way.

VIII.

Communication from the Right Hon. the EARL OF FIFE, *relative to his Plantations**.

SIR,

Plants above 100 acres a year.

I request you will lay this letter before the Society for the Encouragement of Arts, &c. as I feel it my duty to convey any information to them, respecting my plantations, from the grateful sense of the honour they have done me. I have continued every year, since I last wrote to the Society, to plant above one hundred acres: my plantations now, in the counties of Banff, Aberdeen, and Murray, amount to about thirteen thousand acres. I have always recommended to planters to be very sparing in pruning trees.

Trees should be very sparingly pruned.

I have the pleasure to observe, that on the highest grounds in Duff-House Park, even where exposed to the sea, by cutting down firs and other trees, where they interfere with each other, the oaks and other close-grained timber trees rise vigorous and healthy, and will be very valuable, the oaks in particular. The silver fir and larch also grow to a great size. I was under the necessity of cutting down two silver firs and larches, where they prevented the growth of other trees; I directed them to be sawed up—The boards of the larch have been made into tables, and are

Oaks.

Larch wood handsome.

* Trans. of the Society of Arts for 1807, p. 1.

very

Silver fir planks.

very handsome. Those of the silver fir have been used as flooring to two rooms in Delgany Castle, where the fir had decayed, and are remarkably white and finely polished. The trees in question were about forty years old.

Dimensions of a silver fir at 50 years.

There was a very high wind the 25th of December last, which blew down a great many trees upon my estate. Particularly a silver fir in the woods on the low grounds near Duff-House, which appeared to be well sheltered. It was planted by me in the year 1756, and had a most venerable appearance. The dimensions were as follow, as attested to me, viz.

	Ft.	In.
Length of the trunk from the surface of the ground, until divided in five limbs - -	7	0
Girth at surface of ground - - -	9	7
Girth immediately below where the limbs set off	8	6
The five limbs are all of the same height, except No. 1, which divides into two branches before it reaches the top. These are only a few inches shorter than the others, which are 42 feet 6 inches from where they leave the trunk, the length of which is 7 feet, therefore, when added together, the height of the tree, is - -	49	6
No. 1. Measure of girth where it sets off from the trunk - - - - - - -	5	3
And at the distance of 8 feet divides itself into two large branches.		
No. 2. Girth where it sets off from the trunk	4	0
And at the distance of 23 feet 4 inches from starting, measures 2 feet.		
No. 3. Girth at starting - - - -	3	10
This, and the two other branches, No. 4 and 5, gradually decrease towards the top.		
No. 4. Girth at starting from trunk - -	3	7
No. 5. Girth at ditto ditto - - - -	3	3

Pineaster.

The tree alluded to has a great deal of wood in it, which I have ordered to be manufactured for different purposes. There are pineaster larger, but their wood I conceive not to be so fine. The other trees are thriving and well-fenced.

Little

Little trouble is occasioned by keeping the fences in repair. I do not recommend the planting of acorns, but rather procure them from nurseries, at two or three years old.

Seedling larch. I think seedling larch thrives best when planted in moors, and this also thins the seed beds, from which so many may be taken and transplanted into nurseries, and planted out the second year after.

Scotch firs. Hard woods. I raise very few Scotch firs, as I buy them from nurserymen, at ten-pence per 1200. I continue to have nurseries of all the different hard woods, near my plantations, and which I find answer better than what are purchased from nurserymen. In general they are planted too near each other in their nurseries, and not being removed in time, the roots are seldom so good, which I think I have stated in my former letters.

I am, Sir,

Your most obedient humble servant,

FIFE.

IX.

Remarks on the Advantages derived from Plantations of Ash Trees, by David Day, *Esq, of West-hill, near Rochester* *.

Former account of planting ash trees. IN the first volume of the Society's Transactions for 1783, page 109, will be found a detailed account of the experiments which Mr. Day had made to the years 1779 and 1780 in planting Ash trees; the present account points out their subsequent management.

Mr. Day has deposited with the Society a minute account of the expenses to which the following statements refer, and which may be inspected at the Society's house.

* Abridged from Trans. of Society of Arts for 1807, p. 4. The silver medal of the Society was voted to Mr. Day.

SIR,

SIR,

IF you think the following information, relative to my plantations of ash trees, likely to be of advantage to the public, I wish to lay it before the Society of Arts, &c. The Rewards I received from the Society have stimulated me to exertions in this line, and I have been very successful. I have declined all business but that of raising ash trees for my own amusement, and for improvement of the landed interest; and I flatter myself, that I know it as well as any man in the kingdom. I am so certain of the success attending ash plantations, that I am willing, on landed security being given me, to advance any sum as far as thirty thousand pounds, on having the execution of such improvements under my own inspection, either jointly, or on the owner's account. I have travelled over a considerable part of England, and was sorry to see such a waste tract of land as Bagshot Heath, when I know it might be improved by cultivation or planting, as has really been done at Farnham, and many other commons in the kingdom. Where there are, at present, wild or uncultivated woods, I would recommend to grub up the old wood, and either put the land in tillage, or plant it properly with fresh wood, which would produce four times as much both in timber and underwood.

Stimulated to exertion by the rewards from the society.

Bagshot heath might be improved.

Wild or uncultivated woods.

I have made from the underwood of some of my plantations 94*l*. per acre, at only ten years growth, and I am now falling some plantations, of which the underwood alone will produce me 150*l*. per acre, exclusive of the expense of falling. It cannot be expected that noblemen or gentlemen, brought up in expectation of possessing large estates, can have a knowledge of improvements like the executive man, and they are deterred from them by the impositions they meet with in attempting their execution. Few servants will exert themselves properly in improvements without having an interest in it themselves: But my plan is no speculation; I know from long experience it will yield an ample profit to the persons who engage in it with attention. When the plantations are once put in order, they require but little to be done afterwards, and, therefore,

Profit of plantations of ash.

Valuable portions for daughters.

therefore, are good estates for parents to give their female children, as the wood will always find its value from the buyers, when ready for falling, without any trouble or expense to the owner.

I am, Sir,

Your obedient servant,

DAVID DAY.

Statements and profits.

Mr. Day next proceeds to a detailed account of the expenses and receipts on various plantations, of the profits from which the following is a summary.

2 acres in	30 years produced a clear profit of		135	1	8
-	-	-	254	1	4
1½	-	20	121	16	3
7	-	10	88	14	9
6½	-	22	108	1	10
¼	-	24	27	11	2
5	-	23	501	2	7
¼	-	24	19	14	5
2	-	10	23	19	3
2	-	25	59	18	11
¾	-	28	73	8	1
11	-	19	56	8	9
6	-	23	56	6	9

These 6 acres were planted with ash and chesnut; and the 11 acres mentioned in the preceding line were in the hands of a farmer as a tenant, whose cattle, being permitted to graze among the plants, did them much damage.

Method of raising ash trees.

The following is Mr. Day's method of raising ash trees, given in his own words.

Methods of raising Ash Trees.

Choice of seed.

I carefully procure, from good straight well-proportioned ash trees, the ash keys, as they are commonly called, or pods containing the seed, betwixt Christmas and the middle of February. Having, as soon as the ash keys are collected, prepared a hole in the ground, about three or four feet deep, I lay a bed of sand, a few inches deep, at the bottom of the hole; upon that I place a layer of ash keys, about

Keeping it.

about two inches thick; these I cover with sand about the same thickness, to preserve the keys from heating, and then proceed with alternate layers of the keys and sand till the hole is full. They are suffered to remain in this state till the beginning of the month of March of the following year, when they should be taken out for sowing. The keys will be found in a swelled state, ready for vegetation. The land being properly prepared, drills should be made in it as for sowing pease, and the keys laid regularly therein, and covered up with earth.

Sowing.

In about six weeks the young plants will appear above ground, and should be kept perfectly clear from weeds by hoeing.

Hoeing.

In the month of March of the next year they should be planted out in rows, a foot wide, and the plants placed three or four inches asunder in the row. In this state they are to remain for two or three years, when they will be in a proper condition for planting out into the land, where they are to remain.

Planting out.

For planting out where they are to remain, the land being previously well ploughed the preceding autumn, and a good loamy soil, not too wet or stiff, the ground is to be opened by the plough into drills about two feet apart, and the plants placed in each other drill or row, so that the rows of ash are four feet apart, and the plants in the drill two feet asunder. The drills should be 10 or 12 inches deep. A man, who sets the plants, places each upright in the drill, draws the earth to it with his foot, and treads it well in. Where a plant with a larger root than common is found, the man with a small hoe or pricker makes a hole within the drill, a little deeper than usual, to hold the plant, but this is not often necessary.

Final planting.

A statute acre will contain 5400 ash plants, and one man can plant 1000 or 1200 plants in a day.

The intermediate row between the plants may be either set with beans or potatoes, or may be left open advantageously to serve as a drain to keep the young plants dry.

In the second year the plants should be stubbed, or cut close to the ground with a bill; the produce serves for bavins or fire wood, and pays the expense of rent and cutting

Stubbing the plants.

Crops. ting. From the stubs thus left in the ground the regular crops of ash are produced, and are fit for falling every ten years.

The ash plants are usually fallen betwixt Christmas and March, and the wood sorted into poles of three denominations; viz. best, second, and third hop-poles; beside stakes, edders, and bavins. The bavins will amply pay the expense of falling. The best hop-poles are worth, at present, forty shillings per hundred, the second quality twenty-five shillings, and the third ten shillings. Stakes and edders are about two shillings and sixpence the hundred.

Uses of the wood.

Other uses. When the plants remain uncut for twelve or fourteen years, the ash plants are fit for other purposes, such as wood proper for wheelrights, and broad hoops for coopers, beside hop-poles, &c. as before-mentioned.

Nurseries. When the plants have been two years in the nursery beds, and ready for planting out, they are worth from six shillings to ten shillings the hundred according to their quality.

The ash plants I raised from 1763 to 1778, were	442484
I raised and sold from 1778 to 1807 -	156320
I have now ready for sale - - -	126096
Total of plants I have raised	724900

Cheapness of planting. I will engage, provided the land is prepared by my directions, to plant ash for one third less money than by any other mode of cultivation yet known; and for all plants that die in such a case, provided they are in new plantations, I will give plants gratis to replace them the succeeding year.

X. *Chemical*

X.

Chemical Examination of a Sparry Iron Ore, sent to Mr. GUYTON *by* BERGMAN. *By Mr.* COLLET-DESCOTILS *.

THE analyses of iron spar, that have been lately published, having exhibited results considerably different from those obtained by Bergman, it was to be wished, that some of the species on which that celebrated chemist had operated might be subjected to a fresh examination. In fact this was the only method by which it could be known, whether these differences arose from the composition of the ores themselves, or from mistakes in the analysis. Mr. Guyton, who had received from the Swedish chemist a small specimen of the very ore, that had been the principal subject of his examination, having the goodness to break off some pieces from it, and entrust them to me to analyse, I have executed the task with all the attention I could possibly pay to it. The small quantity of iron spar I had at my disposal, it being only 388 cent. [60 grs.], and the method I employed, not allowing me to ascertain the proportion of the volatile principles, I confined myself to the investigation of the nature and quantity of the fixed; and I conceive it necessary, to relate at large the means I employed, that the chemical reader may be enabled to judge of the degree of confidence to be placed on my results; previously giving a brief description of the specimen on which I operated.

Recent analyses of iron spar contradictory to Bergman's.

A piece of the same ore examined.

Its specific gravity, taken by Mr. Guyton, was 3·693.

Its physical characters.

Its colour was brownish yellow.

It was scarcely translucid.

Its crystallization was a little confused: its laminæ very small, and a little twisted.

This ore, reduced to powder, was dissolved with effervescence in sulphuric acid diluted with water; and I took care to employ no more than was necessary, so that the liquor was without excess of acid. Some insoluble matter remained, weighing 1 decig. [1·544 grs.], which was found to be silex.

Dissolved in sulphuric acid.

* Annales de Chemie, Vol. LVIII, p. 149.

The

Evaporated and redissloved.

The solution, on several successive evaporations, afforded crystals of green sulphate. Only a few small scales of sulphate of lime were formed. The last portions of liquor, affording no more crystals, were added to the crystals that had been separated, and the whole diluted with a quantity of water more than sufficient for their complete solution.

Sulphuretted hidrogen added.

Water of sulphuretted hidrogen occasioned no precipitate in this solution: it merely destroyed its transparency, as is the case in solutions of iron that contain but a small quantity of red oxide.

Precipitated by hidrosulphuret of ammonia.

The hidrosulphuret of ammonia, afterward added, occasioned a very copious black precipitate, which was separated by the filter, and washed in cold water.

Precipitate dissolved in aqua regia.

The precipitate detached from the filter was treated with aqua regia. I neglected to burn the filter, and treat the ashes in the same way, which may have occasioned a little loss of the metallic principles; for this very fine precipitate easily insinuates itself into the paper.

Solution decomposed by carbonate of potash.

The nitromuriatic solution diluted in water and filtered was decomposed by saturated carbonate of potash. The ferruginous sediment was redissolved, while still wet, by weak acetic acid, and by successive evaporations the acetate of iron was entirely decomposed. The sediment, collected on a filter, was dried with a red heat, and weighed 188 cent. [29 grs.]. The colourless liquor, separated by the filter, was decomposed by saturated carbonate of potash: it did not become turbid, and it was added to that, which arose from the decomposition of the nitromuriatic solution. From this carbonate of manganese was soon thrown down by boiling, which, when washed, and dried at a red heat, was converted into brown oxide, and weighed 7 cent. [1·08 gr.].

Oxalate of ammonia added.

The liquor separated from the metallic precipitate obtained by the hidrosulphuret of ammonia was mixed with a small quantity of oxalate of ammonia, which did not occasion in it any sensible precipitation. On evaporating afterward, a white sediment formed, which was separated. This sediment, heated in a small porcelain capsule, burned with a blue flame, and left a residuum, which, after being heated red hot, was found to weigh 2 cent. [0·308 gr.]. It had all the characters of lime.

The

The clear liquor was then evaporated to dryness in a platina crucible, and the residuum heated red hot. The ammoniacal salts being expelled, there remained a salt of the weight of 21 cent. [3·243 grs.], which was sulphate of magnesia. This quantity of 21 cent. gives at least 77 milli. [1·189 gr.] of earth, supposing the crystallized sulphate to contain 19 per cent of base; for that which has been heated red hot must have lost some of its acid, and it is necessary to add a little to the solution, to make it crystallize.

Solution evaporated and heated red hot.

On reducing the products above-mentioned to hundredth parts, we shall have

Component parts.

Fragments of quartz - - -	2·58
Red oxide of iron - -	48·45
Brown oxide of manganese - -	1·80
Lime - - - - -	0·52
Magnesia - - - - -	1·98
	55·33

The remainder is carbonic acid, water, and loss.

From this result it appears, that Bergman did not examine with sufficient care the nature of the earthy principles contained in the iron spars he analysed; and it is very probable, that he examined other ores, in which magnesia was contained in still larger proportion, and mistaken by him for lime *.

Magnesia mistaken for lime.

XI.

Chemical Examination of the Alum Ore of Tolfa, and the Earthy Aluminous Schist of Freyenwalde. By Mr. Klaproth †.

Alum, a substance so indispensable in dyeing and several other arts, is a triple salt, composed of sulphuric acid, alumine, and potash, with an excess of acid. It is obtained

Alum an artificial production from ores.

* In the last number of our Journal, p. 314, an analysis of two varieties of iron spar was given, which corroborates the fact of magnesia having been mistaken for lime.

† Journal des Mines, No. 117, p. 179. First published in the Berlin Chemical Journal, vol. VI.

obtained from various earths and stones, which contain the elements necessary to its formation in a more or less perfect state, and are included under the name of alum ores. Thus the alum of the shops is an artificial production.

Native.

Nature it is true presents us with alum completely formed in some volcanic countries, but it is in so small a quantity, as to be altogether insignificant compared with the great demand for it. Among the native alums of volcanic countries that of the alum grotto at cape Miseno, near Naples, is particularly to be distinguished. This is continually efflorescing on the inside of the cavern in small tufts, composed of little, short filaments of a silky lustre, sometimes intermixed with granular crystals. From the results of my examination, which have been published some years, it is well known, that the greater part of this native salt is a perfect alum, that is to say, it has from nature not only the sulphuric acid and earthy base, but likewise the third essential constituent principle, potash.

At Cape Miseno.

Alum of the ancients.

It appears, that the alum we now use was not known to the ancients; and that the *alumen* of the Romans, as well as the στυπτηρια of the Greeks, was a native sulphate, arising from the decomposition of pyrites, and consequently not differing from their *misy* and *sory*.

First made in the Levant.

The art of extracting and preparing alum came to us from the Levant. The most ancient of the alum works known to us is that of Rocca in Syria, now called Edessa; whence the term *alumen Roccæ*, vulgarly rock alum. All the alum used in Europe in the middle ages was brought from the Levant.

Roch alum.

Introduced into Italy.

In the fifteenth century some Genoese, who had learned in the Levant the mode of fabricating it, were fortunate enough to discover ores of it in Italy, and to extract it from them. John of Castro is recorded in history as the first, who discovered the ore of Tolfa. To this discovery he was led by the large quantity of holly growing there; as he had observed in the Levant, that the mountains from which alum was taken there were covered with this shrub.

Holly abundant on aluminous soils.

The manufactures of this salt succeeded so well and so speedily in Italy, that pope Julius II prohibited its importation

tation from the Levant, because it annually drew large sums of money to Turkey. This prohibition increased the prosperity of the Roman alum works.

The following is a brief account of the method employed at Tolfa, near Civita Vecchia. The ore is blown up with gunpowder: it is separated from the pieces of the rock, that adhere to it: it is calcined in furnaces, nearly in the same manner as lime is burned: in six or seven hours, being sufficiently calcined and friable, it is taken out, and laid on pavements of a long shape, surrounded with walled trenches: on these it is laid in heaps of a moderate height, which are watered for forty days with water from the trenches. The ore being thus decomposed, it is boiled in large caldrons; and when the water is saturated to a certain point, it is poured into the crystallizing pans; where, after it is cold, it deposits the alum in large crystalline masses. Method of making it at Tolfa;

Alum is obtained in a very different manner at Solfaterra, near Puzzuola. Here nature acts synthetically. Fumes pregnant with sulphurous and sulphuric acid are continually issuing from little crevices in the volcanic soil of this place, the former of which deposit a concrete sulphur; the second gradually penetrate the ancient lavas, which are of an argillaceous nature, combine with their alumine, and thus form an alum ore, which afterward affords by lixiviation and crystallization a very pure alum. and at Solfaterra.

In the sixteenth century the art of fabricating alum spread into several parts of Europe; after it had been discovered, probably by accident, that various sorts of argillaceous schists, impregnated with carbon or bitumen, and subsequently termed aluminous schists, would furnish alum when properly treated; and the alkali, which did not naturally exist in them, was added during the process. The first works of this kind established in Germany appear to have been those of Commotau in Bohemia, and Schwensal in Saxony. Introduced into other parts of Europe. Germany.

Subsequently, that is in the beginning of the last century, an alum manufactory was commenced at Freienwald, in Brandenburg. At present it belongs to the grand Orphan School at Potsdam, and furnishes annually four hundred tons of alum. Manufactory at Freienwald.

 The

Preparation of the ore.

The aluminous schists, from which alum may be obtained, must undergo a process preparatory to their lixiviation. In the aluminous schists properly so called, which are hard, of a stony texture, and contain a great deal of pyrites, the preparatory process consists in roasting. But for the softer alum ores, such as that of Freienwald, exposure to the air is sufficient. When the ore is extracted from the mine, it is placed in large heaps, sloping to a ridge like the roof of a house, and left exposed to the open air for a year or more. When its decomposition, which is particularly promoted by damp air, is sufficiently advanced, it is distributed into long flat troughs, and lixiviated. When the water is sufficiently saturated with the salts, which are sulphate of alumine and sulphate of iron, it is carried to the manufactory, and boiled in leaden caldrons, till the proof liquor taken out becomes on cooling a crystalline mass of the consistence of honey. During the long boiling of the lixivium, the greater part of the sulphate of iron is decomposed, the iron passes to a higher degree of oxidation, in which state so much of it cannot be dissolved in sulphuric acid, and it is deposited in the form of brown oxide. When the lixivium is sufficiently boiled down, it is carried to the settling troughs, and as soon as it has grown clear by standing a little, it is drawn off into other troughs, where it is mixed with the quantity of potash necessary for making it into alum.

Lixiviation.

Oxidation of the iron.

Potash added.

This obtained from soap-boilers refuse.

At Freienwald, as at most alum works, they use for supplying the alum with this potash the saline mass obtained from soap manufactories, where soap is made with an alkaline lie and muriate of soda, by boiling the spent lie to dryness. The muriate of potash contained in this saline mass is decomposed the instant it is mixed with the aluminous lixivium: the potash unites with the sulphate of alumine, and forms alum, which can no longer continue in solution in the concentrated lixivium, and is precipitated in the form of small crystalline grains, known by the name of alum meal. The muriatic acid, thus set free, lays hold of the oxide of iron, and prevents its falling down with the alum.

Other matters might be used.

Instead of the saline mass from the soap-makers, matters containing sulphate of potash might be employed, as the residuum

residuum left after the distillation of nitric acid, glass gall, &c. The alum meal is washed with cold water, redissolved afterward in a small quantity of boiling water, and lastly drawn off into large wooden vessels, where it is left to crystallize slowly. Crystallization.

I shall now proceed to the proper object of this essay, namely, the chemical analysis of the alum stone of Tolfa, and of the earthy aluminous schist of Freienwald. Analysis of the alum ore of Tolfa

I. *The Alum Ore of Tolfa.*

The alum stone of Tolfa in its natural state contains the three essential constituent principles of alum, considered as a triple salt; sulphuric acid, alumine, and potash. The earth in which it is found is probably of volcanic origin, and has been altered and whitened by the vapour of sulphuric acid. In this it exists in irregular veins, and in nodules. The harder and heavier it is, the richer in alum it is presumed to be. Some naturalists, as Monnet and Bergman, have supposed it contained sulphur, which was afterward converted into sulphuric acid by the process of roasting. But Dolomieu and Vauquelin have shown, that this acid is ready formed in the ore, which will be farther confirmed by what I shall say. Contains all the essential parts of alum. Acid ready formed in the ore.

The alum stone employed in my analyses was of a pearl gray, that is, gray with a violet tinge; in amorphous masses; dull, with a few shining points, or having very little lustre; of an unequal fracture approaching to shelly; a little translucid on the edges; hard, not adhering to the tongue, and heavy. Physical characters of this ore.

A. Two hundred grains were strongly roasted in a small retort with its proper apparatus. An aqueous liquor passed over, highly loaded with sulphuric acid, and accompanied with a smell of sulphurous acid, but without a particle of sulphur. The loss of weight was twenty nine grains. Roasted.

B. Two hundred grains were gently heated, so that the loss of weight could proceed only from water expelled. This loss was six grains. Water expelled.

C. *a*. Two hundred grains were reduced to fine powder, mixed with twice as much carbonate of soda, and the whole subjected to the action of a fire, at first moderate, but afterward Fused with soda,

muriatic acid added,

and diluted with water.

terward increased so as to fuse it. The mass when cold had the appearance of a white enamel. It was well powdered, muriatic acid poured on it to supersaturation, and evaporated to dryness. The residuum, mixed with water and diluted, left behind silex, which after being heated red hot weighed 113 grains.

Part of the solution precipitated by muriate of barytes;

b. The muriatic solution was divided into two parts. Into one of these was poured a solution of muriate of barytes, and sulphate of barytes was precipitated, which after being heated red hot weighed 50 grains; indicating 16·5 grs. of concrete sulphuric acid.

the other by ammonia.

c. The other half was precipitated by ammonia, which threw down the alumine. This when purified, washed, and roasted, weighed 19 grains.

The ore heated with nitrate of barytes, dilute sulphuric acid added, and precipitated by ammonia.

D. A hundred grains of the ore were mixed with 200 grs. of crystallized nitrate of barytes, and heated red hot. The mass was pounded, mixed with water, and supersaturated with sulphuric acid. After evaporating till the saline mass was moderately dry, it was diluted in water, boiled, neutralized with ammonia, and filtered. The liquor being evaporated, and the residuum heated red hot in a platina crucible, left seven grains of sulphate of potash, which included four grains of pure potash.

Its component parts.

According to this 100 parts contain

Silex - - - -	56·5
Alumine - - -	19
Sulphuric acid - - -	16·5
Potash - - -	4
Water - - - -	3
	99·

These as given by Vauquelin,

These component parts are the same in kind as those found by Mr. Vauquelin, who gives them as follows:

Silex - - - -	24
Alumine - - -	43·92
Sulphuric acid - - -	25
Potash - - -	3·08
Water - - - -	4
	100·

The

The difference between these analyses in regard to the respective quantities of the several component parts must have arisen, no doubt, from a difference in the composition of the specimens. differ in their quantities.

II. *Earthy Aluminous Schist of Freienwald.*

The mineral that furnishes the alum of Freienwald owes its origin unquestionably to the vegetable kingdom, and appears to be produced by an alteration of brown coal. It forms a considerable stratum amid the alluvial formation at Freienwald, which is traversed by galleries for its extraction. At coming out of the mine it is of a brownish black, tender or friable, and very slightly shining. Its fracture in the great is imperfectly slaty; in the small, earthy. When rubbed it takes a lustre inclining to that of wax. It belongs to that species of the argillaceous genus, that is designated in the systems of mineralogy by the term aluminous earth *(alaunerde)*. This mineralogical term must not occasion it to be confounded with the simple substance known by chemists under the name of earth of alum *(alaunerde)*, and it is to prevent this mistake I here employ the denomination of earthy aluminous schist. Alum ore of Freienwald of vegetable origin.

Hitherto this mineral, as well as the true aluminous schist, has been considered as a clay impregnated with bitumen and pyrites. It is indeed true, that the earthy schists, and still more those that have the consistence of stone, very frequently contain pyrites: but such ores afford only a very ferruginous alum, and are consequently less fit for the fabrication of this substance, than for that of vitriol. Mistakenly supposed to contain bitumen and pyrites.

The following experiments, made on alum ores of the first quality, will show, that the sulphur they contain is not combined with the iron in the state of pyrites; but that it appears to form a peculiar combination with carbon. The sulphur combined in a peculiar way with carbon.

A. *a.* A thousand grains of the ore, in the state in which it was extracted, were put into a phial with twenty ounces of distilled water, and boiled for an hour; when the liquor was filtered off, and the residuum lixiviated. What passed through the filter was colourless, did not perceptibly change blue vegetable tinctures, and had a vitriolic taste. The ore boiled in water.

b. Half

Half the solution decomposed by muriate of barytes,

b. Half of this was decomposed by a solution of muriate of barytes, and sulphate of barytes was formed, which, after being heated red hot, weighed 23 grains. This precipitate being separated from the liquor, prussiate of ammonia threw down another of prussiate of iron, weighing 40 grains.

the other half by oxalate of ammonia.

c. To the other half oxalate of ammonia was added. It became a little turbid, and assumed a pale yellow colour, which probably arose from a small quantity of oxalate of iron. It then gradually grew clear again, a precipitate falling down, which, after having been heated red hot, weighed 2·5 grains, and was found to be lime contaminated with iron.

Proportions of the sulphates of lime and iron determined.

Thus what the ore had yielded to the water, in which it had been boiled, consisted of sulphate of lime and sulphate of iron, the proportions of which may be determined as follows. A thousand parts of ore produced 46 parts of sulphate of barytes, which contain 15·18 parts of concrete sulphuric acid. Of these 7 parts are required to neutralize the 5 parts of lime; and thus, including the water of crystallization, we may admit 15 parts of gypsum, or sulphate of lime, in the ore. The 8·18 remaining parts of sulphuric acid with 8·5 parts of iron will give about 18 parts of vitriol of iron at the state of decomposition.

The ore boiled with carbonate of soda and precipitated by muriatic acid.

B. Two hundred grains of ore, and 400 grains of dry carbonate of soda, were put into water, and boiled. The liquor when filtered was of a very deep blackish brown colour. Muriatic acid was gradually poured in, which afforded no indication of sulphuretted hidrogen gas; but a muddy sediment was formed, of a blackish brown colour, and occupying considerable space, which, when collected on a filter and dried, weighed twelve grains. Heated in a platina crucible, it burned, without emitting any sensible smell of sulphur, and left behind one grain of white alumine.

Ore digested in muriatic acid.

C. Two hundred grains were digested in muriatic acid. The slightest indication of sulphuretted hidrogen gas was not observable, either by the smell, or by holding against the mouth of the vessel paper, on which I had written with solution of acetate of lead. The acid appeared to display

but

but little action on the ore. On pouring nitric acid on it drop by drop, nitrous gas was evolved, and the black colour of the ore changed to brown. The filtered solution was of a golden yellow; and muriate of barytes threw down from it a copious precipitate. This, which was sulphate of barytes, being collected and heated red hot, weighed 54 grains.

Nitric acid added, and muriate of barytes.

D. *a*. A thousand and two grains of the ore, not yet freed from the humidity it contains in the mine, were put into a glass retort furnished with a pneumato-chemical apparatus. Two hundred and twenty cubic inches of gas were evolved, which was a mixture of sulphuretted hidrogen gas and carburetted hidrogen gas. If a candle were brought into contact with it, it took fire, and burned with a blue flame. When shaken in a vessel containing water, half of it was absorbed. A solution of lead, poured into the water impregnated with it, afforded a precipitate of a deep brown, which was sulphuret of lead.

Ore distilled, gave out sulphuretted and carburetted hidrogen.

b. The fluid that passed over weighed 133 grains. It was aqueous, yellowish, and turbid with slight flocks of sulphuretted carbon. Its smell was that of sulphuretted ammonia, diluted with a great deal of water. Litmus paper, that had been reddened by an acid, it turned blue, and it emitted a white vapour, on bringing near it a glass rod wetted with fuming muriatic acid. A drop was let fall into a solution of lead, and the metal was precipitated brown. It was neutralized by a few drops of muriatic acid, and became slightly milky. On being filtered and evaporated two grains of sal ammoniac were obtained.

The fluid contained sulphuretted ammonia.

c. The residuum left in the retort weighed 750 grains. It had the appearance of a black coally powder. Being burned on a test it left 90 grains, which were the carbon consumed.

Residuum contained carbon.

d. The fifth part of the remaining 660 grains, or 132 grains, was roasted with twice its weight of caustic soda. The mass when cold was of a greenish brown, and gave a light green tinge to the water with which I mixed it. I supersaturated it with muriatic acid, evaporated, diluted it again with water, and filtered. The silex was left behind. This, after being heated red hot, weighed 80 grains.

Silex precipitated from a portion of it.

e. The

Alumine.

e. The solution that had passed through the filter I precipitated by carbonate of potash, washed the precipitate, and boiled it in a lixivium of potash, which became loaded with alumine. This earth being precipitated by muriate of ammonia, washed, and heated red hot, weighed 32 grains.

Sulphate of lime.

f. The brown residuum, that remained in the alkaline lixivium, was dissolved in sulphuric acid, and evaporated to dryness. During the evaporation sulphate of lime was deposited, which, carefully collected, weighed two grains. The dry mass was strongly roasted, and then lixiviated.

Oxide of iron.

The oxide of iron, collected on the filter, was dried, moistened with a little oil, and heated red hot in a close vessel, when it yielded 14·5 grains of oxide of iron attractable by the magnet.

Magnesia.

The remaining liquor, decomposed during ebullition by carbonate of potash, gave some slight indications of carbonate of magnesia.

Water expelled by heat.

E. *a.* One hundred grains* were put into a small glass retort, which was placed on a sand heat, and the fire cautiously increased, lest any gas should be evolved, or any perceptible decomposition occasioned, and that nothing but water might be raised from it. The quantity expelled was 21·5 grains. It had a very slight opal tinge, and a very faint smell of sulphuretted hidrogen. A very slight coating of sulphur too was deposited in the neck of the retort.

The ore burned without flame or smoke.

b. The ore being dried was burned on a test; when the combustion proceeded without flame or smoke, and emitted but a slight sulphurous smell. The loss in weight, which was 45 grains, represents the quantity of sulphur and charcoal burned, and perhaps too a small portion of water, that was left in the ore.

Magnesia precipitated.

c. The residuum was dissolved in a mixture of 200 grains of sulphuric acid, and 400 of water, evaporated to dryness, and kept at a strong red heat for half an hour. The residuum was lixiviated, filtered, and precipitated with ammonia, when 0·5 of a grain of magnesia were obtained.

* This is apparently an errour of the press. According to the proportions of the constituent principles given at the end, it must have been two hundred grains. *F. Ed.*

d. The

d. The liquor was evaporated to dryness, and the residuum heated till no more white fumes were expelled. What remained weighed 4·5 grains. It was a neutral salt, formed of a mixture of sulphate and muriate of potash. As this last salt must necessarily have been completely formed in the ore, we may admit too, that the potash of the former was not free in it, but formed a real component part of it in the neutral state. Till experiments on a larger scale shall have enabled me to determine more accurately the proportions of these two salts, I shall reckon that of sulphate to that of muriate as three to one.

Sulphate and muriate of potash.

F. The results of the experiments above given will serve to rectify some of our chemical ideas respecting the earthy aluminous schist of Freienwald, and those of a similar nature.

Corrections of received notions.

1. In their composition there is carbon only, but not bitumen; for they afford no bituminous oil by distillation, and when roasted in open vessels they burn like charcoal without flame or smoke.

The ore contains carbon, but no bitumen.

2. The sulphur of the ore, which becomes oxigenized during its exposure to the air, and thus forms the sulphuric acid necessary for the production of alum, is not combined in it in the state of pyrites, exclusively of any pyrites mixed with the ore accidentally, but is intimately united with the carbon, and this in a manner with which we are not yet well acquainted. With the best lenses we cannot discover the smallest atom of pyrites in the ore, either in its natural state, or after it has been triturated and washed through the sieve with care*. In this state of combination with carbon the sulphur is protected against the solvent power of alkalis, and gives no sulphuretted hidrogen gas with muriatic acid.

The sulphur united with carbon in a peculiar way: and not forming pyrites.

G. As to the determination of the respective proportions of the constituent principles mentioned, there is some difficulty

Difficult to determine the proportions of

* I have observed in several coal-mines, particularly those of Anzin, a fact, that has probably some connexion with this mentioned by Mr. Klaproth. The coal that produces fire damps does not contain any pyrites, at least perceptible to the eye; and in the same places the coal that contains a great deal of pyrites is wrought without the least danger.

Fire damp not from pyrites.

Sulphur and carbon.

culty in it, arising chiefly from the intimate union between the carbon and the sulphur; as these two substances cannot be separated in the dry way, without new gaseous compounds being formed.

Alumine.

Sulphur.

The essential parts of the mineral, as an alum ore, are alumine and sulphur. The ordinary processes of analysis give us directly 160 parts as the quantity of alumine in 1000 of the ore. The sulphur not being obtainable in a separate state, we must deduce its quantity from that of sulphate of barytes obtained in treating the ore by nitric acid. According to what has been said (in C), 1000 parts of the ore produced 270 of this sulphate. From this quantity 46 parts are to be subtracted, which were furnished by the vitriol and gypsum, and 20 by the sulphate of potash, admitting 15 of this sulphate in 1000 of the ore. Thus there remain but 204 parts of sulphate of barytes produced by this sulphur: but 204 parts of this salt contain 90·75 of sulphuric acid of the specific gravity of 1·85, or 67·5 of concrete acid, which are produced by the oxigenation of 28·5 of sulphur. And if (according to E *b*) the sum of the sulphur and carbon may be taken at 225, on deducting 28·5 for the sulphur we shall have 196·5 for the quantity of carbon.

Carbon.

1000 p. ore might produce 216 alum.

H. Admitting that 1000 parts of crystallized alum, decomposed by muriate of barytes, produce at a mean 945 of sulphate of barytes, we shall find, that the 28·5 of sulphur contained in 1000 of ore may afford 216 parts of alum, provided the proper quantity of potash be added. The component parts of the ore, that produce them, are not a fifth part of the mass.

Less obtained from defects in the process.

If the quantity of alum obtained, or even that might be obtained in the manufactories, be much less than I have mentioned, this arises from the imperfection of the process employed to produce the efflorescence of the ore during its exposure to the air. The oxigenation of the sulphur, and consequent formation of sulphate of alumine, takes place only on the surface of the lumps, and of course the greater part of the ore remains undecomposed.

Component parts of the aluminous schist.

I. From the preceding experiments we may infer, that 1000 parts of the earthy aluminous schist of Freienwald contain

Sulphur

Sulphur	-	G.	-	28·5
Carbon	-	G.	-	196·5
Alumine	-	D. *e.*	-	160
Silex	-	D. *d.*	-	400
Black oxide of iron, with a slight trace of manganese	-	D. *f.*	72·5	
From which subtract for the vitriol			8·5	
			64 -	64
Vitriol of iron	-	A. *c.*	-	18
Sulphate of lime	-	A. *c.*	-	15
Magnesia	-	E. *c.*	-	2·5
Sulphate of potash	-	E. *d.*	-	15
Muriate of potash	-	E. *d.*	-	5
Water	-	E. *a.*	-	107·5
				1012·

It is very possible however, that the quantity of some of these component parts may be capable of being determined with more accuracy. As to the excess of about one per cent, which the sum total shows, this may be considered as of little importance in an analysis like the present.

XII.

On the Effects of Galvanism on Animals. In a Letter from Mr. JOHN TATUM.

DEAR SIR,

MY two papers on galvanism having met with an insertion in your Journal, induces me to send a third, containing galvanic experiments, some of which I presume will be new to most of your readers, as I believe no one has performed them but myself.

Galvanic experiments.

After having killed two frogs, one by electricity, and the other by immersion in carbonic acid gas, and dissected them in the usual manner, I endeavoured to excite them by a galvanic trough of 50 plates, containing 350 inches surface, but no muscular contractions ensued. I did not (as is

Two frogs killed, one by electricity, the other by fixed air, could not be excited.

is generally the case) confine my experiments to the inferior parts only of the frog, but made them on the superior also.

The first moistened with oximuriatic acid, without effect.

I then moistened both upper and lower extremities of the frog killed by electricity with oximuriatic acid, and immediately applied the positive and negative wires of the above trough, but with as little success.

But six hours after it was convulsed by a single pair of plates.

Having left them on the table, to attend to some other experiment in another room, I did not return to remove them till about six hours after the experiment, when I was much surprised to observe the head of the latter frog appear more healthy than when I left it. It being late in the evening, I began to lament, that I had emptied and cleaned my trough, as I wished to try its effects a third time; but from the appearance I was tempted to try the effect of a pair of zinc and silver plates of $1\frac{1}{2}$ in. diameter, and the convulsive motion produced by this small power far exceeded my most sanguine expectation.

Two mice killed.

I also killed two mice, one by dividing the vertebræ of the neck, the other by confining it under a bell glass containing about a pint of atmospheric air. The first mouse was powerfully excited by a pile of 60 pairs of zinc and copper plates moistened with solution of muriate of soda. But the same pile produced so small a degree of motion in the second mouse, that I can scarce say whether it moved or not.

The one by suffocation not excited.

Two frogs galvanized in water

After having performed a variety of experiments before a numerous company with four troughs of 106 pairs of plates, containing 5360 inches surface, I placed the positive and negative wires in a glass jar of water, in which were two large frogs. The instant both the wires touched the water, the frogs betrayed the greatest signs of uneasiness, so much that some gentlemen requested me to remove the wires. I complied with their request, but observed, I had every reason to believe they would not survive. The result was, that on the next day (being left in the water) they appeared very languid, and on the second day they were dead.

died in two days.

Perhaps, Sir, I may presume too far, in submitting my theory or opinion on the above experiments; but if I err, I am open to conviction, and shall esteem it a particular favour to be corrected by any of your scientific readers.

I conceive,

I conceive, Sir, that animals possess a certain portion of excitability, which, by the application of various powerful stimuli, such as galvanism and electricity, produce muscular motions both in the living and dead animal; and if too great a portion be applied, it finally exhausts the excitability, and produces death. But the excitability may also be destroyed by depriving the animal of those things, which are calculated to increase or replenish it: I farther conceive, that the excitability is in proportion to the oxigen the animal or parts of an animal may possess; and if animals are deprived of life by the above means, I am inclined to think little or no motion can be produced by the most powerful stimuli with which we are acquainted. This theory, Sir, I think will account for the results of the experiments I have detailed.

Accounted for on the principle of exhausted irritability.

Thus with respect to the two frogs killed, one by electricity, the other by depriving it of oxigen; the excitability being destroyed in both, it could not be exerted. But the excitability, I conceive, was in some measure restored in the first by its absorbing oxigen from the oximuriatic acid or the atmosphere, after being a few hours exposed to their action. The first mouse, being suddenly deprived of life, still possessed a greater quantity of oxigen than the second, which was killed by depriving it of the vital principle by degrees; and thus it was easily excited, while the latter was not.

The two frogs killed by galvanism may be accounted for on the same principle as the frog killed by electricity.

As this is committed to paper in a hurry, I flatter myself the candid reader will draw a veil over any imperfections. Permit me, Dear Sir, to subscribe myself,

Yours truly,

JOHN TATUM.

53, Dorset Street,
July 21, 1808.

Remark.

These experiments appear by no means sufficient to prove, that death is caused by the deprivation of a peculiar principle of excitability, according to the ingenious theory of Brown;

Brown; still less, that oxigen is that principle, an hypothesis I believe first broached by Girtanner. Much indeed must be done, before we can venture to establish any theory on so abstruse a subject, as that of vitality still remains: a subject on which it is not of so much importance to multiply facts, as to describe those that present themselves with accuracy, and with attention to every concomitant circumstance even of the minutest kind. C.

XIII.

On the Structure and Uses of the Spleen. By Everard Home, *Esq. F. R. S.* *

Communication between the stomach and circulation through the spleen.

In bringing forward a fact of so much importance, as a communication between the cardiac portion of the stomach and the circulation of the blood, through the medium of the spleen, I shall not take up the time of the Society by offering any preliminary observations, but state the circumstances which led to the discovery, and the experiments by which the different facts have been ascertained.

Stomach during digestion separated into two portions.

During the investigation of the functions of the stomach, (in which I have been lately engaged,) it was found, that, while digestion is going on, there is a separation between the cardiac and pyloric portions, either by means of a permanent or muscular contraction †. This fact placed the process of digestion in a new light, and led me to consider in what way the quantities of different liquors, which are so often taken into the stomach, can be prevented from being mixed with the half digested food, and interfering with the formation of chyle.

Fluids chiefly contained in the cardiac portion, and carried out of the stomach without reaching the pylorus.

Pursuing this inquiry, I found, that the fluids are principally contained in the cardiac portion, and the food that

* Philos. Trans. for 1807, p. 45. The president and council of the Royal Society adjudged the medal on Sir Godfrey Copley's donation, for the year 1807, to Mr. Home, for his various papers on anatomy and physiology, printed in the Philosophical Transactions.

† See our Journal, p. 15 of the present vol.

has

has reached the pyloric portion is usually of one uniform consistence, so that the fluids, beyond what are necessary for digestion, would appear to be carried out of the stomach, without ever reaching so far as the pylorus. To ascertain the truth of this opinion is the object of the present paper.

The lymphatic vessels of the stomach are numerous, but they are equally or more so in the other viscera. Many circumstances appeared to render it probable, that the spleen is the route by which liquids are conveyed. The more I considered the subject, new reasons in favour of this opinion crowded on my mind, so as almost to enforce conviction, and made me set about devising various methods, by which its truth or falsehood might be established.

Lymphatics appear inadequate to this.

Perhaps by the spleen.

The first point to be decided was, whether the liquids received into the stomach do escape in any considerable quantity, when prevented from passing out at the pylorus.

Fluids can escape from the stomach, without passing the pylorus.

This was ascertained by the following experiment, made October 31, 1807, with the assistance of Mr. Brodie, Mr. W. Brande, and Mr. Clift.

The pylorus of a small dog was secured by a ligature, and a few minutes afterwards five ounces by measure of an infusion of indigo in water, of the temperature of the atmosphere, were injected by the mouth into the stomach. At the end of half an hour the dog became sick, and brought up by vomiting 2 ounces of a nearly colourless fluid. The dog was immediately killed, and the different parts were examined. The pylorus was found completely secured by the ligature, so that nothing could pass in that direction. The pyloric portion of the stomach was found empty and contracted; the cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal membrane. Of the five ounces of water thrown into the stomach, two were brought up by vomiting, and one only remained; two ounces had therefore escaped in the course of half an hour. As the stomach contained two ounces of solid food at the time the experiment was made, it is reasonable to suppose, that there was also

This proved on a dog.

also some liquid in it, and in this case the whole quantity that escaped must have exceeded two ounces. On examining the external covering of the stomach, and along the course of the vasa brevia, where the absorbents usually pass, none were discovered, so that these vessels were not at that time carrying any liquid.

Not by the lymphatics.

The spleen turgid with an aqueous fluid.

The spleen was turgid, unusually large, and its external surface very irregular; when cut into, small cells were every where met with containing a watery fluid, and occupying a considerable portion of its substance. This appearance, which I had never seen before, made me inquire, if it had been taken notice of by others, and endeavour to ascertain the circumstances, under which it is produced. The following statement contains the information, which I have received on this subject.

Malpighi's notion of the spleen.

Malpighi appears to be the first anatomist, who had any particular knowledge of the structure of the spleen. He describes its capsule, and a network which pervades every part of the substance. He mentions a number of small glands, which are hollow, and surrounded by arterial zones, but he had never been able to trace any venal branches into them. He believed, that there was a cellular structure in the spleen containing red blood, interposed between the arteries and veins; this led him to adopt a theory, that the network was muscular, and by its action propelled the blood, so that there was a systole and diastole in the spleen, as in the heart.

Stukely.

Stukely, in his Gulstonian lecture, has very closely copied Malpighi, without giving any additional information.

Cuvier.

Cuvier, the latest writer on this subject, in his *Leçons d'Anatomie comparée*, corrects the errour of Malpighi respecting the nature of the network, which he states to be composed of elastic ligament, and says, that there are small corpuscles, the use of which is unknown, and which disappear when the blood vessels are minutely injected.

The corpuscles of Cuvier, or glands of Malpighi, contain a fluid, after drinking largely.

In the course of the present investigation, I have examined the spleen after death, under the ordinary circumstances, and have found the appearances described by CUVIER. I have also examined it frequently immediately after the stomach had received unusual quantities of liquids, and in that state

state have found invariably, that the corpuscles of Cuvier, which were the glands of Malpighi, are distinct cells, containing a fluid, which escapes when the cells are punctured, and renders their membranous coat visible; so that it would appear, that the distension of these cells is connected with the state of the stomach, and therefore only takes place occasionally; and that the elastic capsule, by which the spleen is surrounded, adapts the organ to these changes in its volume.

Farther examination of the spleen

On examining further into the structure of the spleen, in which I have been materially assisted by Mr. Brodie, the following facts have been ascertained.

Arteries ramify in the coats of the cells.

In the spleen of the bullock, horse, and hog, the cells, when the arteries and veins are injected with coloured size, are seen to have numerous arterial branches ramifying in their coats, but no venal ones, which confirms the statement of Malpighi; and when the cells are empty and contracted, and the blood-vessels filled to a great degree of minuteness, the appearance of cells is entirely lost, as stated by Cuvier.

Intermediate parts but few arteries.

When the cells were in a distended state, their cavities in a great many instances were very distinct, having been laid open in making a section of the spleen. The intermediate parts of the spleen are but sparingly supplied with arterial branches, and the smaller ones do not appear to have any particular distribution.

Veins more numerous than the arteries, and radiate from the cells.

When the veins only are injected, their branches appear more numerous, and larger than those of the arteries, making the whole substance of the spleen of a red colour. They appear to arise from the outside of the cells, going off at right angles to their circumference, like radii. Where the injection has not been very minute, they are seen to arise at so many points of the capsule; but where the injection has got into smaller branches, their number is so much increased, that they appear to form plexuses round the cells.

Splenic vein 5 times as large as the artery.

The trunk of the splenic vein, compared with that of the artery, when both are filled with wax, is found to be in the proportion of five to one in its size. This was ascertained both by an accurate measurement of their diameters, and by

by weighing half an inch in length of each in a very nice balance; the disproportion between them is greater, than between corresponding veins and arteries, in other parts of the body.

Experiment with madder.

Having acquired this knowledge of the internal structure of the spleen, I made the following experiment with a decoction of madder. This substance was employed, from the animals who feed on it having their bones tinged red, so that there can be no doubt of its colouring matter being carried into the circulation of the blood. I was much disappointed on seeing the colour of the decoction, which, instead of being a bright red (the tinge communicated to the bones), was of a dirty brown. The same gentlemen assisted me, as in the former experiment.

Decoction injected into the stomach of a dog, with the pylorus tied up.

Nov. 8, 1807, seven ounces of a strong decoction of madder were injected into the stomach of a dog, immediately after the pylorus had been secured. At this time the dog voided some urine, which was limpid and colourless. In 42 minutes, two ounces of a yellowish fluid were brought up by vomiting. In 18 minutes more the dog vomited again; what came up proved to consist of $3\frac{1}{2}$ ounces of solid matter, and 3 ounces of liquid. In 15 minutes afterwards, 5 ounces of the decoction were injected, which remained quietly on the stomach for two hours and a quarter, at the end of which period the dog was killed. In the act of dying he made water, in the quantity of two ounces, of a dark muddy colour. This was saved, and afterwards compared with the remaining liquid in the stomach, which it exactly resembled. On examining the connections between the stomach and spleen, none of the absorbent vessels were apparent, more than in the former experiments. The pyloric portion of the stomach contained about two ounces of half digested food, but no liquid. The cardiac portion contained four ounces of liquid, and half an ounce of solid food, so that the act of vomiting, which appeared, at the time, a sufficient exertion to have completely emptied the stomach, had brought up no part of the contents of the pyloric portion, and had not even completely emptied the cardiac portion. In this experiment, without making allowance for any liquid in the stomach, prior to the decoction

After $1\frac{1}{4}$ hour urine resembled fluid in the stomach.

State of the internal parts.

$\frac{3}{4}$th of the liquid had escaped.

tion of madder being injected, one fourth part of the quantity thrown in had escaped. The cells of the spleen were more distinctly seen than in the former experiment, particularly at the great end.

Although there was every reason to believe, that the colouring matter of the madder had been conveyed into the urinary bladder, yet so muddy and indistinct was the colour, that it was by no means completely ascertained. I therefore resolved in my future experiments, to make use of some colouring substance, the presence of which could be detected in a very diluted state, by means of a chemical test; and I requested Mr. W. Brande, of whose assistance I have before availed myself, to point out the substances best fitted for this purpose. He immediately suggested, that rhubarb was a substance, which he had made use of as a test to ascertain the presence of alkali, and therefore had no doubt, that the caustic alkali would prove a test of rhubarb. This substance has also another advantage; it is well known to pass very readily by the kidneys, without being decomposed.

Madder not well calculated for the experiment.

Rhubarb suggested.

The following are the results of experiments made with rhubarb, to ascertain the best modes of detecting it in the urine and blood, and the time it takes to pass from the stomach to the urinary bladder.

Trials of its sensibility.

Five drops of tincture of rhubarb, added to 3 ounces of water, are found to strike an orange tint when the test is added, which does not take place when the rhubarb is more diluted.

In water.

Six drops of tincture of rhubarb, added to three ounces of serum, are readily detected by the eye, but the colour is not heightened by applying the test; the alkali contained in the serum being sufficient to strike as bright a tint, as that quantity of rhubarb can receive from the addition of alkali.

In serum.

When tincture of rhubarb is mixed with blood just taken from the arm, its colouring matter is afterwards found both in the serum and in the coagulum.

In blood.

When blood is drawn from the arm of a person, who has taken rhubarb in sufficient quantity to affect the urine, the serum is found to have a slight tinge from it, equal to that, which one drop of tincture of rhubarb gives to half an ounce of serum when added to it.

Gets into the blood when taken.

Half

Its effects on the urine,

Half an ounce of tincture of rhubarb, diluted in $1\frac{1}{2}$ ounce of water, taken in the interval between meals, did not pass off by urine in less than an hour, and even then was not in sufficient quantity to be discovered, till the test was applied.

The same quantity was taken immediately before a breakfast consisting of tea. In 17 minutes, half an ounce of urine was voided, which when tested had a light tinge. In 30 minutes another half ounce was made, in which the tinge was stronger; and in 41 minutes a third half ounce was made, in which it was very deep. In an hour and ten minutes 7 ounces were voided, in which the tinge of rhubarb was very weak, and in two hours twelve ounces were voided, in which it was hardly perceptible.

and fæces.

In $6\frac{1}{2}$ hours the rhubarb acted on the bowels, and gave a decided tinge to the fæces; the urine made at the same time had a much stronger tinge, than what was voided at one hour and ten minutes.

Gets into the urine by two different channels.

In this experiment, the rhubarb appeared to have escaped from the cardiac portion of the stomach; and in two hours ceased to pass through that channel; but was afterwards carried into the system from the intestines, and again appeared in the urine.

Experiment repeated with similar results.

This experiment was repeated on another person; the rhubarb was detected in the urine in 20 minutes. In 2 hours the tinge became very faint; in 5 hours it was scarcely perceptible; in seven hours the rhubarb acted on the bowels; and the urine made after that period became again as highly tinged as at first.

Prussiate of potash suggested.

It was suggested by a chemical friend, that the prussiate of potash might be a better substance than rhubarb, for the present experiments, since the solution of one quarter of a grain in two ounces of water becomes of a blue colour on the addition of the acidulous muriate of iron.

Not to be detected in the blood in small quantities.

To determine this point, one quarter of a grain was dissolved in two ounces of serum, but no blue colour was produced by the addition of the test, nor did this effect take place till the quantity of the prussiate was increased to a grain; so that minute quantities of the prussiate of potash, or at least of the prussic acid, may exist in the blood, without being detected by adding solution of iron.

The

The effects of rhubarb on the urine, and the different parts of the blood, having been thus ascertained, a third experiment was made, in which that substance was employed, and I had the assistance of the same gentlemen as in the others.

Ex[illegible] with [illegible]

On November 17, 1807, at 35 minutes past 11 o'clock, five drams of a mixture of tincture of rhubarb and water, in the proportion of a dram to an ounce, were injected into the stomach of a dog, the pylorus of which was secured. At 20 minutes past one, two ounces of fluid were brought up by vomiting: ten minutes afterwards, another ounce of the mixture was injected, as were nine drams more at half past four o'clock. The two last portions were retained, and at eight o'clock in the evening the dog was killed.

on a dog.

On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state, so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

None of the lymphatics of the stomach distended,

but the spleen turgid, and bladder full.

This urine, tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubarb had been taken three hours by the mouth, and in other respects resembled it.

Rhubarb in the urine;

When the spleen was cut into, the cells were particularly large and distinct. A portion of it was then macerated in two drams of water for ten minutes in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained off and tested by the alkali, and immediately the reddish brown colour was produced in the centre, and no where else, but in less than a minute it began to diffuse itself, and extended over the whole.

and in the spleen.

A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place in it whatever.

None in the liver.

In this experiment the rhubarb was detected in the juices of the spleen as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there

Could not [illegible] passed thr[illegible] the comm[illegible] absorbent [illegible]tem.

through the medium of the common absorbents carrying it into the thoracic duct, and afterwards into the circulation of the blood.

The inquiry to be pursued.

The discovery of this fact I consider to be of sufficient importance to be announced to the Society, that, when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiments, to prosecute the inquiry.

XIV.

On the Purification of Lemon Juice. In a Letter from a Correspondent.

To Mr. NICHOLSON,

SIR,

Purification of lemon juice:

A READER of your valuable publication submits to your judgment the following methods of purifying lemon juice, which should you think worthy of a place therein, it will oblige

Yours, &c.
PHILOCHEMICUS.

by nitromuriate of tin;

1st, Take of nitromuriate of tin, (prepared by dissolving the metal in a mixture of two parts nitric, and one muriatic acid) one dram, lemon juice one quart; after standing forty eight hours filter through white paper.

or charcoal.

2nd, Take of finely pounded and well burned charcoal one ounce; lemon juice one quart; mix, and after standing twelve hours filter through white paper.

Second method perhaps preferable.

The latter method seems preferable, as there is nothing employed, which can in any degree injure the juice, the charcoal being perfectly insoluble.

In the former, perhaps some of the solution of tin may pass the filter, though it is most probable it precipitates along with the mucilage and extractive matter, which are so combined, that one cannot be precipitated without the other; however should any pass, the quantity must be so small, as to render it of little consequence.

SCIENTIFIC

SCIENTIFIC NEWS.

I AM informed by Dr. Forbes of Edinburgh, that he is engaged on a translation of Pliny's Natural History, which is to be accompanied with such notes and illustrations, as may be necessary to elucidate the context, a Life of the Author, and a Preliminary Dissertation on the Origin of Natural History, and on its progress and gradual improvement, from its infancy to its present state of comparative maturity. New translation of Pliny's Natural History.

He observes, that the thirty-seven books of the Natural History of *Caius Plinius Secundus* may with propriety be regarded as the *Encyclopædia* of antiquity, since its very inquisitive and industrious author has collected all the facts recorded by every Greek and Roman writer previous to his own time, concerning the animal, the vegetable, and the mineral kingdoms, and detailed in a clear and luminous arrangement all that the accumulated experience of past ages had ascertained, relative to the nature of *animals* and *vegetables*, to *meterology*, *astronomy*, *botany*, *medicine*, *chemistry*, &c. Pliny's work may be divided into three parts, *geography*, *natural history*, and *materia medica*. Of his geographical inquiries his strictures on the interior parts of Africa are perhaps the most important. He derived the sources of his information on this subject from the Carthaginians; and from what he has recorded respecting the natives and productions of those regions, it is evident, that the ancients were much better acquainted than the moderns are with this quarter of the globe, which from recent events, and from the consequences likely to arise from a great act of national justice, deservedly excites in this country no small share of public interest. The *materia medica* exclusively occupies fifteen books of the *Historia Naturalis*, and constitutes a very curious and instructive department of the author's investigations. It cannot be denied, that Pliny discovers his ignorance in particular points, and that he has recorded with solemn gravity many absurd

New translation of Pliny's Natural History.

absurd fables and anile stories. But perhaps he might have used the language of Quintus Curtius, "*Equidem plura transcribo quam credo;*" and we know, that he occasionally discovers a proper degree of scepticism on various points, which came under his review, and severely rebukes the vanity and self confidence of the Greek authors, from whom he derived his information. Yet, notwithstanding all the censure to which he is obnoxious on the score of credulity, his eloquent and instructive history will ever be regarded as an imperishable monument of its author's indefatigable industry and Roman spirit. Pliny's Natural History is indeed to be considered an invaluable treasure, more especially on account of its containing an infinite number of excerpts and observations illustrative of the various subjects of which the author treats, extracted from the books of many ancient writers, whose works have perished through the injuries of time. It may therefore appear surprising, that no English translation of this admirable performance has been offered to the public for more than two centuries. It is the present translator's object to supply, to the best of his abilities, this *desideratum* in English literature. One great object, which the translator will keep in view in his notes and illustrations, will be, to accommodate Pliny's descriptions of animals, plants, and minerals to the nomenclature of the *Systema Naturæ Linnæi.* This, he is abundantly aware, will prove by much the most difficult part of his labours; and he despairs of executing it with full satisfaction either to the public or to himself. But as in the present state of natural history a translation of Pliny would not be well received without some account of the *synonyms*, he enters on the task in the hope of being able to contribute in some measure toward its accomplishment. The translation thus enlarged must extend to six or seven volumes in octavo, and will be published either in separate volumes successively, or when the whole shall have been finished, as future circumstances may render it advisable.

INDEX.

A.

B.

bited

Dead

Gordon,

INDEX.

L.

M.

N.

Nicholson,

INDEX.

Vallât,

END OF THE TWENTIETH VOLUME.

Stratford, Printer, Crown-Court, Temple-Bar.